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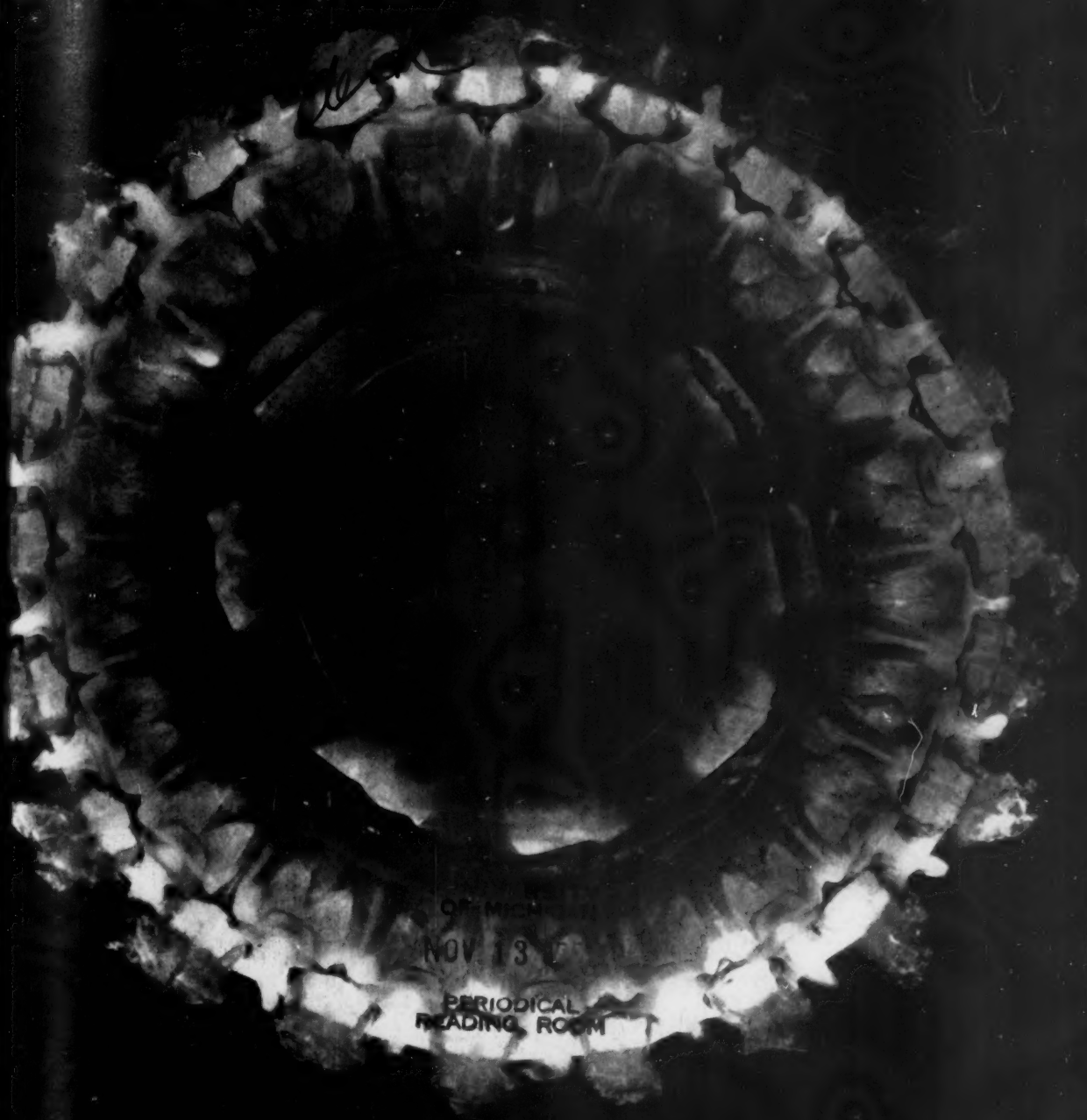

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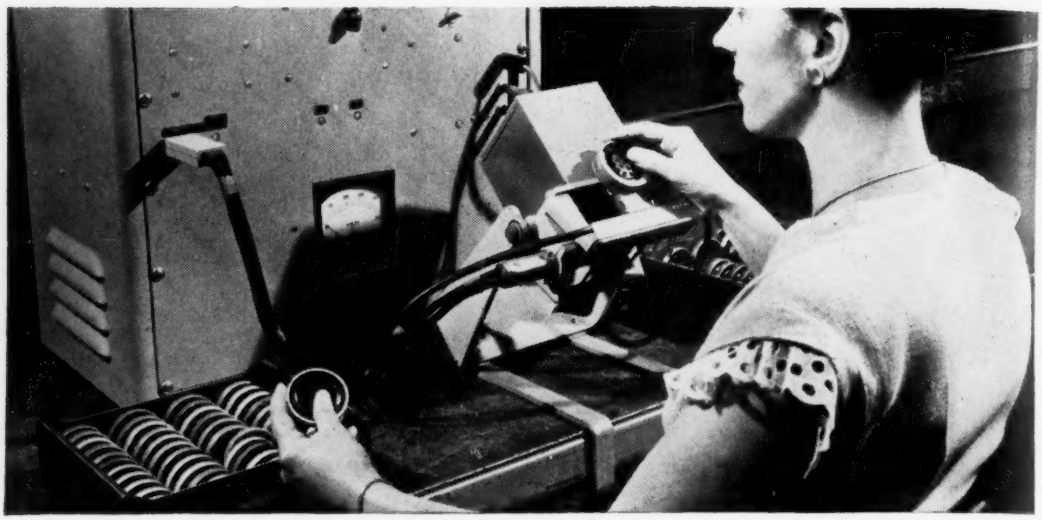
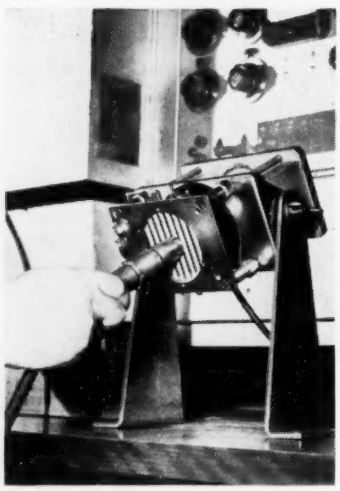


This Western Electric employee mounts a transmitter in the test fixture which is swung down to face an artificial mouth at 45-degree angle, just as transmitters are held in use. More than a million transmitters are tested each year.

This mouth speaks to millions



At Bell Laboratories a scientist employs a condenser microphone to check the sound level from another type of artificial mouth, used in transmitter research.

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Science and Technology

(From the Month's News Releases)

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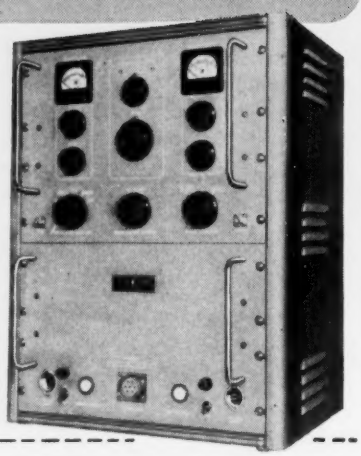

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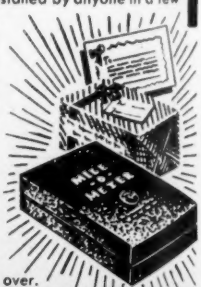

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Glacial Water Levels in the Thames-Willimantic River Valley*

RICHARD J. LOUGEE and ADRIAN W. VANDER PYL

This article offers a new approach to the late-glacial history of southern New England and to fundamental principles of glaciology. Mr. Vander Pyl did the field work, using surveying and interpretive techniques employed by Dr. Lougee in twenty-five years of glacial studies in North America. He is a graduate of Clark University, where Professor Lougee is the geomorphologist in the Graduate School of Geography.

ONE of the principal drainage lines of eastern Connecticut is a river of many names, which is known in various parts as the Middle, Willimantic, Shetucket, and Thames (Fig. 1). The head of this river system is on a divide 612 feet above sea level, two miles north of the Massachusetts-Connecticut state line, and its outlet 50 miles farther south is through the Thames Valley to New London on Long Island Sound. The valley is alternately narrow and wide, and a number of sharp turns divert the drainage from one to another of several parallel trenches in the bedrock. Much of the river course pays little attention to rock structure, and frequently cuts directly across it as it threads a way through rock gorges and glacial deposits at levels 200–400 feet below the upland surface of Connecticut. Where southerly movement of glacial ice took advantage of structural lines or surface irregularities, it developed in the bedrock noticeable trough shapes and defiles, and at least locally overdeepened the valley. Between Norwich and New London the deep trough of the Thames ends the river system in a true fiord 10 miles long.

So many anomalies in the course of this river suggest that the present-day valley may be a composite of several different preglacial valleys. How such diversified topography acquired integrated drainage is a problem that has its key in events of the Ice Age to be discussed here.

Previous Terrace Studies

Terraces in the Thames Valley with distinct ice contacts attracted the attention of USGS topographer Frederick P. Gulliver, who described them in 1899.[1] Gulliver discovered that they contained delta structure, indicating some kind of late-glacial

* Based on a paper presented by Adrian W. Vander Pyl at the Annual Meeting of the AAAS, Cleveland, Ohio, Dec. 27, 1950.

submergence. He described them as deltas built into water at the margin of a glacier and the valley wall. This is also the view of the present report. Gulliver noted progressive increase in elevation of the terraces toward the north but was uncertain of the explanation for the same.

In 1929 the Thames terraces were surveyed by Lougee, who calculated a tilt rate of 8 feet per mile along a glacial water plane at sea level near New London and 100 feet near Norwich. Lougee, Antevs, and Sayles explained the Thames and other valleys of southern Connecticut as estuaries of a body of glacial water in Long Island Sound.[2]

In 1930 Richard Foster Flint[3] interpreted the Thames-Willimantic terraces according to a hypothesis of general stagnation of the last ice cap. The terraces, it was thought, were formed in descending order, the highest first, their level controlled by lakes along a margin of ice melting southward across New England to a final stage of stagnation in Long Island Sound. Terraces and water levels alike were assumed to be horizontal, and from this standpoint the state of Connecticut was considered to be outside the limits of any late-glacial crustal deformation of New England.

The original version of the stagnation hypothesis involving deglaciation southward was repudiated in 1931.[4] For the past two decades a modification of it known as "downwastage" has been advocated by Flint and others, who believe that as the last ice retreated northward the surface melted down until the valleys were left occupied by long thin tongues, and these wasted away in a stagnant condition of their terminal portions. As recently as 1949 this explanation was employed in a report on Willimantic Valley terraces by Sidney E. White,[5] supervised and critically reviewed by Kirk Bryan and L. W. Currier. According to White, the terraces around Stafford Springs are kame terraces with high gradients attributable to the original slope of southward-flowing ice-marginal rivers. No general submergence was noted, nor reference made to land tilting. "Porous, stagnant ice masses" were believed to have built deposits of sand and gravel on the floor of the valley after the main body of the ice cap had retired to the north.

FIG. 1. Glacial Thames-Willimantic estuary in eastern Connecticut, extending across the Hobbs Hinge Line.

Thames Valley Terraces

The present investigation was made in 1950 by Vander Pyl in conjunction with other Pleistocene terrace studies by Lougee. The USGS topographic maps of Connecticut with 10-foot contour interval proved indispensable to field work. Aerial photographs allowed stereoscopic study of the terraces, and when particular elevations were needed levels were run from government bench marks.

Inspection of the Thames Valley verified Gulliver's observations of estuarine conditions following retreat of the ice toward the north. Ice-marginal deltas on both sides of the Thames Valley delineate the lobate form of the last ice, and sections show delta structure similar to that illustrated in Gulliver's photographs of topset and foreset bedding in the Groton Naval Base terrace. Steep margins of the terraces overlooking tidewater are partly ice contacts and partly delta lobes. Kettles are commonly present. Heavy cobble which caps the terraces and forms the margins of the kettles and ice contacts is a deposit of torrential streams capable of burying solid masses of the living glacier or flanking the ice margin with volumes of sand and gravel in the briefest time.

Ice-marginal deltas are a common type of terrace in the valleys of New England. They can be observed growing in Alaska in the space between an active ice tongue and the valley wall, where they prograde into water at the ice front (Fig. 2). In Connecticut, as elsewhere, they may be found to terminate a system of southward-sloping hillside channels or less steeply inclined ice-marginal river beds, or "kame terraces." Channels developed at several levels mark repeated discharges which produced overlapping or connecting delta terraces.

Absence of any generally paired matching of deltas on the two sides of the New England valleys, a condition illustrated by the case in Alaska (Fig. 2), makes it appear likely that they were not built by daily supplies of meltwater. Meltwater of present-day glaciers usually drains away inconspicuously beneath the ice to emerge in subglacial tunnels. Channel patterns and topography show that the deltas were built either by land streams or by torrents from ice-dammed lakes in the highlands as these lakes were quickly and repeatedly lowered by retirement of the ice border.

South of Norwich the valley floor is below sea level, but delta terraces continue the evidence of submergence north of Norwich, where additional features appear at levels lower than the deltas. These include: (1) kames and eskers containing plentiful foreset (underwater) bedding; (2) laminated or sometimes varved silts and clays proving the former presence of deep and relatively quiet glacial waters; and (3) low-level gravel terraces attributable to early stages of erosion and free flow of a river on the valley floor.

The Shetucket section of the valley is aligned northwest-southeast, and channeling along the southerly valley wall indicates ice border rivers temporarily working eastward before further retreat let the submergence from the Thames reach an elevation of 200 feet at Willimantic. Deep-water eskers and clays extend past the city. For two miles below Stafford Springs a narrow and rockbound gorge containing *roches moutonnee* on its floor shows that ice passed through and accentuated the tortuous character of the valley, but terraces are present within the gorge, and excellent delta structure appears north of it in terraces at Stafford Springs at 536 feet. From Stafford Springs toward Monson the highway follows one delta terrace after another, and on lower ground in mid-valley there are eskers of coarse gravel, sometimes broad and flat-topped. Several contain clay bands indicating a winter season and deep, quiet water. At State Line Pond a kettle-pitted delta records the submergence at 605 feet.

Termination of the Submergence in Massachusetts

On the drainage divide one is confronted with bedded silts where the railroad crosses the divide at 612 feet. All about and rising to higher levels are steep-sided sandy kames of underwater origin, so that there is no escape from the fact that the submergence extended northward over the divide and into the valley of Chicopee Brook which slopes northward to Monson and Palmer, Massachusetts.

Chicopee Brook Valley has a steep northward descent. Silts and sandy kames mantle the valley floor, and an esker extends for some miles north of the divide. The massiveness and great height of this esker, compared with low eskers south of the divide, imply greater depths of water and stimulated conditions for esker development as drainage southward "climbed uphill" from under the ice to reach the ice front. According to this concept of formation of so-called DeGeer-type eskers,[6] there was a high and thick ice cap to the north, capable of producing a water table within the ice which could impel the subglacial river to flow up-slope and build successive pieces of esker at a tunnel mouth in the deep water of the estuary.

Any uncertainty about the deep-water character of the esker is removed by the fact that a mile north of the present drainage divide the esker grew 150 feet high until it reached the surface of the water, which it registered accurately by forming a

FIG. 2. Ice-marginal delta at edge of Crillon Glacier, Lituya Bay, Alaska, similar to deltas built at the sides of the Willimantic-Thames estuary. Notice contact of delta and live glacier. Lack of a corresponding delta at opposite side of glacier suggests a non-meltwater source, either land stream or ice-dammed lake in the distant highlands. Glacier surface at the horizon is 1,200 feet above sea level in foreground of picture, simulating conditions of a tongue of the last ice sheet in less rugged topography of Connecticut. (U. S. Navy photo.)

flat-topped node, or "esker delta," at an elevation of 650 feet. This proglacial delta on the esker marks a frontal position at a time when the region six miles farther north at Palmer, with elevation about 330 feet, was buried under hardly less than 500 feet of glacial ice, and perhaps double that amount. To the south an estuary extended from the ice front in Massachusetts completely across the state of Connecticut to Long Island Sound.

The esker delta just mentioned is in southern Massachusetts, and is the last known and farthest north of the high-water-level features. Near Palmer, ice-marginal deltas record much lower water levels (660 and 560 feet). A mile southwest of Palmer a deep glacial channel (Fig. 3) leads across the ridges separating Chicopee Brook from the Connecticut Valley to the west, showing that when the ice border reached that point Chicopee Brook Valley held an ice-dammed lake which broke out to the west at an elevation as low as 600 feet. According to this evidence the waters had begun to subside about the time the ice front reached Monson. The 650-foot esker delta in South Monson seems to register the first sign of this subsidence, for it is 10 feet lower than might be expected to fit the profile of deltas farther south. Deltas and eskers north of Monson must belong to lower stages of water than the submergence extending to Monson from the Thames Valley.

The Profile of Deltas

In studies of glacial water levels the problem of crustal warping can be most simply analyzed by construction of a profile of plotted measurements. Such a profile with vertical scale exaggerated 145 times has been prepared (Fig. 4) showing all significant glacial features in relation to the present stream profiles. The profile is drawn straight from Palmer to New London, in a direction twelve degrees west of north. Errors of measurement or interpretation of the water levels not exceeding a few feet in amount have no detrimental effect on the profile, which involves measurements with a vertical range of hundreds of feet. The first of several notable features to be observed in this profile is the continuity of deltaic deposits in a

single long "water plane" from New London to Monson. This water plane is believed to mark a former horizontal water surface that grew in length toward the north as the ice front withdrew in that direction. Heights of the water plane above present streams give approximate depth of the water, which was generally less than 100 feet—if we may disregard the postglacial dissection of deposits forming the floor of the estuary.

The second notable feature is the evidence that at some time after the water expanded to Monson the water plane has been tilted upward toward the north. Moreover, there is a readily recognized grouping of the delta terraces into two separate straight sections that are unequally tilted. Deltas in the southernmost straight series are lowest at New London, where they appear to emerge from below sea level and become higher in elevation northward to the vicinity of Willimantic at an elevation of about 200 feet. This rate of rise is approximately 8 feet per mile, as measured in 1929 by Lougee. Except for temporary ice-dammed lake deposits near Willimantic, which are believed to be tilted in conjunction with other parts of the valley, there is no interruption in continuity of the water plane. Estuarine conditions are continuous past Willimantic, but the more northerly deltas are arranged along another straight section of water plane which rises at the phenomenally steep rate of 18 feet per mile, measured in a northwesterly direction. Where the two straight segments join near Willimantic is the point of crossing of a break in the earth's crust, or "hinge line," to which the name "Hobbs Hinge Line" is herewith applied, in honor of William Herbert Hobbs, long a student of earth lineaments. The dislocation of the two parts of the water plane by the Hobbs Hinge is a record of differences in amount of tilt between individual blocks of the earth's crust amounting to 10 feet per mile.

It is difficult to interpret the continuity of ice-marginal deltas and the prolonged persistence of the estuary as evidence of anything but a formerly straight and horizontal water plane, indicative of a stable condition of the earth's crust during the uncovering of these parts of Connecticut. This former condition is illustrated in Figure 5. The water in the estuary is in general so shallow as to permit no mechanism of constantly declining water levels on the assumption that a tilting movement was in progress during the uncovering. A possibility of very slight movement during the period of expansion of the estuary cannot be denied, but if such took place it left no tangible evidence. Rather, the indications imply long stability followed by active land movement when the ice front reached Monson.

Consequences of the Concept of "Normal Retreat"

The elevation of 650 feet to which the submergence extends northward across Connecticut is sufficiently startling to make the investigator pause and question his own evidence. All the fundamental concepts of glaciology, and the accepted principles of sedimentation, are put to test by these extraordinary conclusions to which they have led us. Naturally questions arise: Has the evidence been correctly interpreted? Is the long valley across Connecticut actually no estuary but only floored with a dissected train of sand and gravel deposited around quantities of rotting ice in a steplike arrangement of little pools filled with delta structure? Are eskers formed in tunnels under motionless ice or in crevasses open to the air, rather than as here interpreted to be overlapping deposits at the mouths of tunnels in a deep-water environment? Do kettle holes and eskers, so numerous in New England, indicate that the ice cap by a process of "downwastage" melted to "extreme thinness" and "dissipated" in stagnant masses along the floors of the valleys?

This quandary was presented with full impact as the authors considered the easier explanations offered by current theories for the much-studied

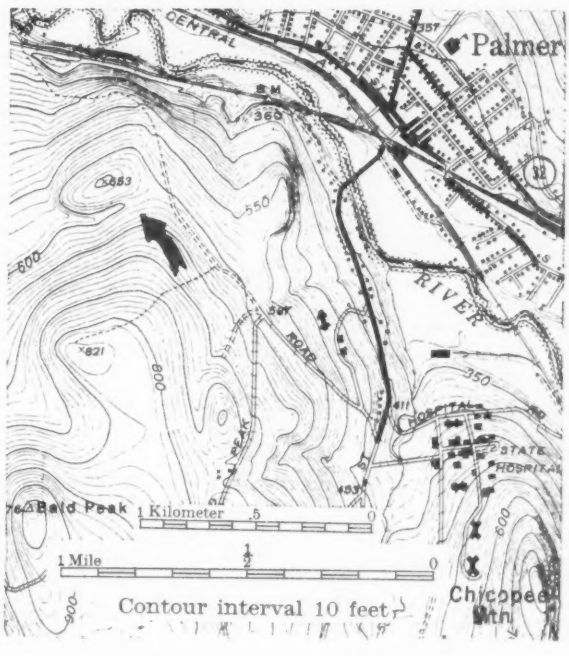

FIG. 3. Drainage channel at 600-foot elevation one mile southwest of Palmer, cut by the discharge westward to the Connecticut Valley of waters from an ice-dammed lake in the Chicopee Brook Valley. (Palmer Quadrangle.)

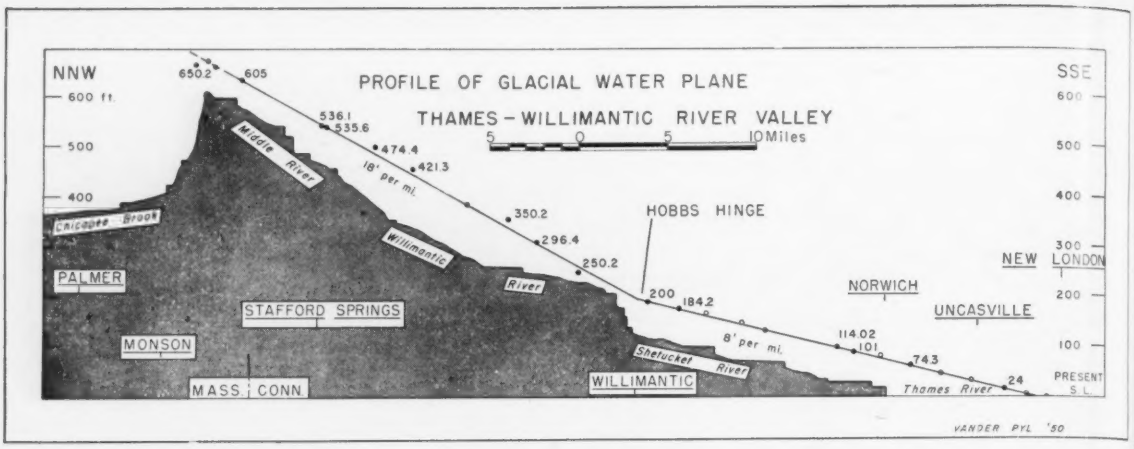

FIG. 4. Delta elevations plotted in profile from New London (sea level) to Monson (650 feet). Solid dots are surveyed elevations; circles, elevations from USGS maps with 10-foot contour. Vertical exaggeration, 145 times.

terrace deposits of Connecticut. A majority of glacial researches since 1930 have explained the New England deposits by disappearance of a thin, rotten, and tunnel-ridden glacier, despite insistence of Alden in 1931,[7] Antevs in 1939,[8] Lougee in 1938[9] and 1940,[10, 11] and Johnson in 1941[12] that a thick and live ice cap disappeared by "normal retreat."

By "normal retreat" the authors mean that the ice margin melted back northward at rates generally exceeding the rate of ice movement southward. Surface depletion, according to this view, was confined to the steeply sloping peripheral zone extending some scores of miles in from the edge of the cap, which was two or three thousand miles across. The high interior, a region of precipitation and nourishment until final stages of deglaciation, was lowered only by radial outward flow at the bottom, where pressure and temperature produced great plasticity. Thus the cap is understood to have maintained its dome shape through the action of gravity making constant adjustments between nourishment, outward flow, and melting in the peripheral zone. Along the ice margin where visible movement would have been greatest—judging by present-day ice caps—melting separated individual blocks from the active mass—a process known as "calving;" but back of a steeply sloping periphery the ice was thick enough to depress the earth's crust in northern Connecticut about a thousand feet.

Conditions are pictured quite the reverse in the concept of "downwastage," and yet this philosophy of ice mechanics has its own logic. Downwasted ice is thought of as "thin ice," and a thin ice cap may be considered impotent to prevent a condition of widespread stagnation from developing in its margins and valley tongues. This supposed stagnant marginal condition will explain kettle holes, eskers, and the host of ice contact deposits just as

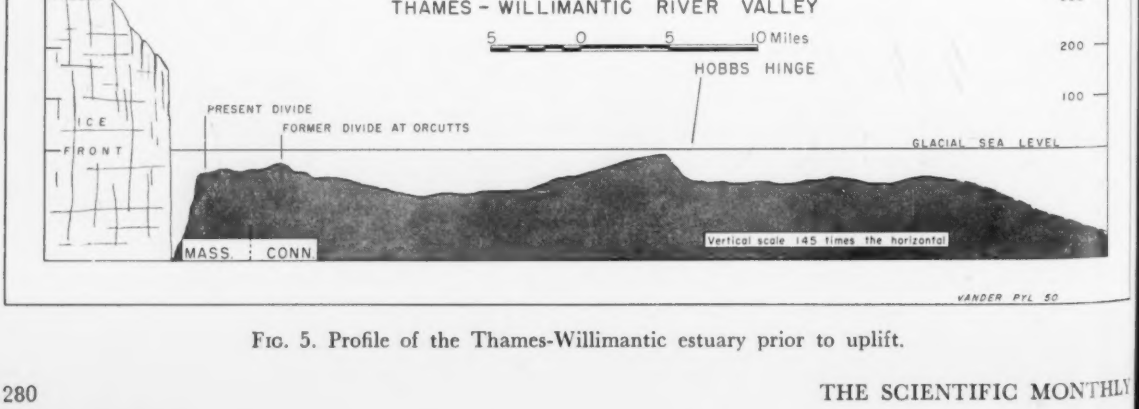

FIG. 5. Profile of the Thames-Willimantic estuary prior to uplift.

well as rapid burial of live ice, provided the almost invariable relationship of these features to torrential glacial gravels is not considered relevant. Certainly a thin ice cap agrees well with the idea that there was little or no crustal depression in Connecticut.

On the other hand, if the concept of normal retreat of a thick ice cap is strictly applied, and evidence for standing water is rigidly interpreted according to the views of fluvioglacial sedimentation employed by the authors, then the state of Connecticut is seen to have been differentially tilted since the Ice Age not less than 650 feet from south to north, and to a degree in the northern part of the state of 18 feet per mile, which is one of the greatest crustal warpings yet measured in North America or Europe.†

Drainage Divides Shifted by Tilting

Critical evidence for testing the opposing philosophies of deglaciation was unexpectedly discovered after much of the field work was completed. Former existence of an estuary should have produced inevitable consequences. Whenever the waters of the estuary subsided a powerful discharge of running water must have been produced across the drainage divide between Chicopee Brook Valley and Middle River Valley. If lowering of the waters resulted from opening of a new outlet in a northerly direction a current should have flowed across the divide northward. But if lowering were due to contemporary land movement in the form of differential uplift or "tilting" it should have lowered the whole estuary and produced a discharge of waters southward from an ice-fed lake at the Monson ice front.

To settle these alternatives the drainage divide was given renewed inspection. Although a railroad cut is shallowly excavated across it, the site was found utterly lacking in evidence that running water, even in the slightest amount, has passed across this divide. Deep-water sediments so thick that they completely bury the bedrock are characteristic of the locality! Absence of evidence of stream flow in either direction at first created a mystery, but this was solved by a return to consideration of the profile of the valley. If the steeply tilted water plane is tipped back to the horizontal position it had when the waters began to subside (Fig. 5), we should expect that the first appearance of the river on the floor of the estuary would be at that place where the estuary had its least depth. The present-day divide is not such a place.

Inspection of the Stafford Springs quadrangle map suggested shallower depths of water over the trough of Middle River Valley south of the present divide. Half a mile south of Orcutts Railroad Station field inspection revealed a 30-foot-deep notch cut by Middle River through a ridge of bouldery till deposits, barely 20 feet below the estuary water plane. This compared with a calculated depth of 45 feet of water over the present drainage divide, although the latter now has the higher elevation. A search immediately south of this notch in the till showed the valley floor for almost half a square mile overspread with gravels that have fanlike structure and a fanlike areal pattern on the contour map (Fig. 6). The surface rises toward the gap in the till. Flatness of this immense fan has been an attraction for a cemetery site, and its location, low elevation, and great size offer reason for believing that it marks the depositing work of the glacial waters after cutting through the first point of emergence of the valley floor. Although it was explained by White and Bryan as produced by residual ice in the valley,[5] the interpretation here presented attributes it to the response of the glacier-fed river to a beginning of active differential land movement when the ice front was no nearer than Monson. Since this locality has no similar deposit connecting with near-by Edson Brook Valley, ice appears to have retired entirely from the head of that valley.

If the events of late-glacial history took place as described, then there has been a shifting of the drainage divide from a former position in Connecticut to a present location five miles farther north in Massachusetts. With differential uplift amounting to 18 feet per mile, shifting of divides was not only a probability, but valleys that now slope southward at rates less than 18 feet per mile may have sloped northward before the tilting. The Schooley Peneplane forming the upland in these regions of Connecticut and Massachusetts, and sloping evenly southward today with a gradient of 15 feet per mile, must have sloped toward the ice center before the uplift!

Beginnings of Integrated Stream Flow

It was natural for integrated stream flow in the Middle-Willimantic-Shetucket-Thames Valley to follow immediately the disappearance of late-glacial submergence. So extensive was the submergence that genetically unrelated drift-mantled valleys were connected by the common flood, and as the waters subsided they still had direct connection

† Tilt rates increase to 24 feet per mile in the upper Ware River Valley of northern Massachusetts. European measurements of postglacial crustal warping are not known to exceed 9 feet per mile in Scania, Sweden (personal communication of E. H. DeGeer).

with the ice front. From an ice-dammed lake which replaced the estuary in Chicopee Brook Valley—a lake extending 30 miles from the Orcutts outlet near Stafford Springs to Spencer, Massachusetts, at the head of the Quaboag Valley—a powerful river flowed southward across the emerging estuary floor. This initial course was consequent upon glacial and estuarine deposits and led from one basin to another with a termination in the Thames. Barring the probability of temporary pools, a continuity of stream flow was almost instantly the result of uplift, and the present stream pattern dates from that event. Intrenchment along this line of stream flow cleared the deposits from several ice-deepened gorges and locally superposed the river profile on rock. Stream terraces outline an inner valley in the glacial deposits all the way to Norwich, but the eroded materials have left no delta above present sea level. Indications are that the glacier-fed Willimantic River flowed on south of Norwich to points beyond the present coastline where there is probably a submerged delta. Postglacial rise of sea level has drowned this channel and the more southerly records of regional uplift, to create the present tidal estuary in the Thames Valley.

Southward discharge of the river from the ice-fed lake stopped when further ice retreat at Monson opened lower outlets westward (Fig. 3). This diversion took place before the present divide was tilted high enough to be eroded by the currents going across it. From such a fortunate circumstance it has been possible to substantiate the former static water plane and the extraordinary steep regional tilt rate.

The Leverett Marine Water Plane

It remains to summarily explain these relationships to the larger problems of New England glaciation. Of first consideration is the problematical nature of the estuarine waters that flooded the Middle-Willimantic-Shetucket-Thames Valley. If the tilted water plane is extended on straight south of New London at a rate of 8 feet per mile beneath present sea level it will project into the floor of Long Island Sound—obviously an impossible condition, since this provides for no outlet to the sea. It is necessary to think of the plane as projecting over the rim of the basin somewhere to the south, regardless of whether the glacial waters were fresh or marine. To accomplish this it is logical to assume that the plane bends to a gentler gradient in a seaward direction and that it may eventually become horizontal. This explanation requires the postulate of another important hinge line in the

FIG. 6. Orcutts till barrier at former divide between Chicopee and Willimantic drainage, and fan built by water from an ice-dammed lake cutting through the barrier. Local eskers were formed earlier in the deep water of Willimantic estuary. (Stafford Springs Quadrangle.)

vicinity of Long Island Sound—probably the eastward extension of the Whittlesey Hinge Line, well known in the Great Lakes.

South of Montauk Point and Block Island there is a submerged plateaulike deposit over 50 square miles in area, facing the ocean basin to the south, which is interpreted to be an ice-fed delta built at the front of the outermost (Ronkonkoma) terminal moraine. If the interpretation is correct, the delta marks a position of sea level at the maximum of the last glaciation. Making allowance for the hinge line, the estuary water plane can be extended southward to reach the position and elevation of the submerged delta and just pass above the submerged rim of the Sound basin. Tilting probably continues farther south to an initial hinge line near the edge of the continental shelf, but the close correspondence in elevation of submerged delta and water plane is a reason for believing this to be a water plane of the sea at the maximum of glaciation, when sea level was perhaps 300 feet, more or less, lower than today. In honor of the late great glacialist Frank Leverett, the name "Leverett

Marine Plane" has been proposed for it. It is the oldest of three glacial marine planes recorded in New England, the other two being the DeGeer Sea of eastern Maine, and the Champlain Sea of Vermont.

The Hobbs Hinge Line

Crustal warping in southern New England has been profoundly influenced by the hinge line so conspicuously crossed by the water plane near Willimantic. Northeast of Willimantic, at Putnam, Connecticut, in the Quinebaug River Valley, and also at Grafton, Massachusetts, in the Blackstone Valley, there are conditions nearly identical to those at Willimantic. Surveyed water planes at those places are sharply bent in a way to produce exceptionally steep tilt rates to the northwest. The alignment of these three widely spaced points is noticeable (Fig. 1), and the tectonic line that passes through them where the crust is broken or bent into two unequally tilted zones is the Hobbs Hinge Line. Extensions of the line northeastward are less certain, for ice mantled the land surface of eastern Massachusetts when the hinge line was most active. However, Crosby and Lougee showed in 1934[13] that a hinge line appears to cross the DeGeer marine plane between Newburyport, Massachusetts, and Hampton, New Hampshire.

If the line is projected southwesterly from Willimantic it will encounter the gorge of the lower Connecticut River. Within this gorge the towns of East Haddam and Moodus, Connecticut, are noted for having the greatest seismicity in all New England. The prevalence of small earthquakes here, and to only a lesser degree near Newburyport, Massachusetts, as pointed out by Dr. Hobbs in 1907,[14] may find an explanation in the presence of the Hobbs Hinge Line near these places.

Hubbard Uplift

Finally, emphasis should be given to the important contemporary land movement when the ice front uncovered Monson. This mighty uplift, following the Leverett Stability, is believed to have been of continental proportions. It antedated the uncovering of the Great Lakes Basins and came long before the Whittlesey Uplift, which is the earliest recorded land movement in the glacial Great Lakes. It has been named the "Hubbard Uplift" for George D. Hubbard, who first detected evidence of it in small lake basins in northern Ohio.[15] Hubbard Uplift is recorded in eastern North America where exceptionally steep tilt rates are characteristic of the early uncovered parts of southern New England, including a deep interlobate zone occupying the Eastern Massachusetts Highlands. When uplift began, the Connecticut Valley as far south as Middletown, Connecticut, and the Boston Basin as far south as Kingston and Taunton, Massachusetts, were ice-covered. Regions covered in Hubbard time record only the gentle tilting of much later Whittlesey Uplift, but regions uncovered early enough and north of the Hobbs Hinge Line to register both the Hubbard and Whittlesey Uplifts show exceptionally steep tilt in their water planes.

The Hobbs Hinge Line appears to have been most active in the Hubbard Uplift, accounting for high tilt of the crust block northwest of it, yet it also functioned in the Whittlesey Uplift, which tilted the DeGeer marine plane north of Boston. Hubbard Uplift took place with a rapidity excellently documented by contemporary lowering of base level at Monson and many other localities along the ice border from Kingston to New Haven.[9]

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Cranks and Scientists

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IT IS unfortunate, though perhaps necessary, that specialists in any branch of human endeavor acquire attitudes and methods of communicating ideas that set them apart from other specialists and from nonspecialists: we must strive as best we can to see that these differences do not form an impenetrable barrier to mutual understanding. The recent controversy centering on Velikovsky is a case in point: the general public as represented by the editors and many of the readers of *Harper's* has failed to grasp the reasons for the scientific rejection of Velikovsky's hypothesis, and many of them may therefore be led to think of scientists as a dogmatic crew, blindly maintaining their own unverified doctrines; intolerant of opposition, and suppressing it by denying free expression to their adversaries. The scientists, in general, have not been aware of the enormities attributed to them. They have not realized that the tempest is over something more than the purely scientific question; therefore, since they know that all other scientists agree with them in the rejection of Velikovsky's hypothesis, they are inclined to consider the question closed and turn their attention to less depressing matters. It is our purpose to defend scientists before the bar of public opinion, and in doing so, perhaps to indicate to the scientists the offenses with which they have been implicitly or explicitly charged.

Any widely known scientist, and particularly any astronomer, receives a considerable stream of letters that he classifies as "crank" letters, which propose, and dogmatically affirm to be true, propositions which the scientist recognizes as preposterous, ridiculous, and contrary to all the recognized truths of his science. Crank letters come from all sorts of people; from some who are extremely ignorant and even illiterate, through those of moderately good education, up to a very few who have the best of academic backgrounds. Most, from whatever educational level, are ignorant of the principles and facts of the field they write about, but a few have acquired an extensive though usually ill-assorted compendium of information. Crank material is not confined to private letters; it appears occasionally in a letter to the editor, in newspaper or magazine articles, in privately printed books and pamphlets, and once in a while in the output of a reputable publisher. Many research scientists collect crank material, just as teachers collect boners, and for precisely the same reasons.

But here we may interpose an objection. Is not the scientist, in rejecting the crank letter, and even in labeling it "crank," just as dogmatic as the man whose theory he rejects? Most scientists feel that they have a real justification for their attitude, but most of them find it hard to defend their position, and quite impossible to convince the cranks that they are cranks. As a result the crank letter is customarily unanswered, or answered noncommittally. After all, even if the crank theory is opposed by accepted scientific theories, the same may be said of almost every new theory in science, and all the great innovations of the past have involved the rejection of previously accepted beliefs, as the crank is usually quick to point out. Furthermore, the scientist is committed to freedom of inquiry, and recognizes that any scientific theory is open to question. There is hardly a theory that is not questioned by some scientist of repute, and in the cases of the few exceptions, it would no doubt be possible to find someone who could be called a scientist and who would take the opposition view. The crank may therefore feel himself unfairly treated: is he not to be permitted to question existing theories unless he is a member of the union, unless he teaches in a university or is a member of the American Blankological Association? No such idea is in the mind of the scientist, and, as he is unwilling to be thought of as partisanly clannish, he

must search further to justify his rejection of crank theories.

It is typical of the crank theories that they contradict many established laws rather than one. If the scientist corrects, modifies, or even considerably alters the laws of one branch of science, the main body of scientific law remains unchanged. Not once in a generation is there an innovation so important that it changes many laws, and can truly be called revolutionary for some branch of science. Cranks propose such innovations every day, and happily assume that they are not merely scientists, but members of that highly restricted group fortunate enough to discover revolutionary new laws. Naturally enough, the odds favor the assumption that anyone proposing a revolutionary doctrine is a crank rather than a scientist, but this does not settle the issue for any individual case.

There is another characteristic of a revolutionary scientific principle in which it differs from the typical crank theory: no matter how widespread its implications, no matter how many previously accepted laws must be modified, all the changes fit into one thoroughly integrated pattern. The scientist respects nature and its laws insofar as they are already known to man. Even when, for the better comprehension of nature, he is forced to modify some of these laws, the only ones changed are those directly connected with his work. If he is a psychologist, he might propose a law that would entail extensive modifications of sociology or biology, but hardly both. The crank, on the other hand, has a theory that he seeks to force upon nature, and he will ignore all facts and deny all theories that stand in his way. The biological crank has the intrinsically harmless theory, for example, that there are winged elephants. Where? For convenience, let us say they are in the next room. If we do not see them, then perhaps we have a curious physical fact, that light rays bend around winged elephants, thus making them invisible; or a curious psychological fact, that winged elephants are good hypnotists and hypnotically persuade us that they are not there; or perhaps a curious sociological fact, that all other men are engaged in a conspiracy to discredit the crank.

We have already said that there is hardly a scientific theory that is not questioned by some scientist of repute. This is so because science is unfinished business, an inquiry into the habits of nature where all the evidence is not in and where much of the evidence that is in has not been digested. Under these conditions there is room for minority opinions, some of which will, no doubt, turn out to be correct. There is a parallel here, though, with horse racing: long shots run in the races, and some will no doubt win. But a sports commentator who expected a long shot to win in almost every race would be open to suspicion. In the same way, the man who accepts one or two scientific "long shots" is perfectly reasonable, but when a man accepts too many of them, his scientific standing becomes suspect. The crank is one who tries to force nature into his own selected pattern; the evidence of strain resulting from this practice is divergence from currently accepted views. In attempting to defend these divergences, the crank may be led to excessive reliance upon minority opinions and the authority of individuals. As a corollary to this, he may also attempt to discredit majority opinion, scientific method, or scientists in general.

The scientist is familiar with the theories that he is proposing to modify and proposes his own hypothesis as an improvement upon them. He is aware of the merits and the failures of the present accepted theory, and his hypothesis normally accounts for all the phenomena for which the existing theory accounts, and for more as well. In those rare cases where the new hypothesis is less successful than the old in handling a few phenomena, it makes up for this by success with a larger number that the old theory could not handle. If the existing theory has led to successful predictions, no successor could be advanced which did not do as much: if the existing theory is capable of mathematical accuracy, the new hypothesis must be so as well.

We may therefore list a number of questions to be asked of revolutionary hypotheses, the answers to which will help us make up our minds as to whether the proposer is a scientist or a crank:

1. Is the proposer of the hypothesis aware of the theory he proposes to supersede?

2. Is the new hypothesis in accord with currently held theories in the field of the hypothesis, or, if not, is there adequate reason for making the changes, reasons of weight at least equal to the weight of the evidence for the existing theories?

3. Is the new hypothesis in accord with the currently held theories in other fields? If not, is the proposer aware that he is challenging an established body of knowledge, and does he have sufficient evidence to make such a challenge reasonable?

4. In every case where the new hypothesis is in contradiction with an established theory, does the hypothesis include or imply a suitable substitute?

5. Does the new hypothesis fit in with existing theories in all fields, or with substitutes proposed for them, to form a world view of an adequacy

equivalent to that of the currently accepted one?

6. If the new hypothesis is at variance with theories capable of prediction or of mathematical accuracy, is the new theory itself capable of such prediction or mathematical accuracy?

7. Does the proposer show a disposition to accept minority opinions, to quote individual opinions opposed to current views, and to overemphasize the admitted fallibility of science?

It is not our primary purpose to examine the merits of Velikovsky, but in defense of his critics it is necessary to point out that he qualifies as a crank by almost every one of these tests, perhaps by every one. Let us consider the tests seriatim:

Test 1. In one sense Velikovsky is clearly aware of the laws he proposes to replace: he is by training a scholar, and prepared to quote names, dates, and page numbers without end. Yet he frequently betrays complete lack of understanding of the laws with which he is familiar by name. To be more specific, he announces in his preface that his hypothesis is to replace those of Newton and Darwin, but thinks that his views are in accord with "modern conceptions of physics." Yet he does not understand Newton's laws, nor even, as pointed out by J. Q. Stewart, the pre-Newtonian celestial mechanics for which Newton's laws provided an explanation: Darwin fails to receive more than a few passing mentions in his book: and Velikovsky's views are not in the least in accord with modern physics. This last misconception arises from applying quantum jumps, and perhaps the uncertainty principle as well, to macrocosmic phenomena. But it is a matter of common knowledge among physicists, and one frequently explained in popularizations of modern physics, that quantum phenomena, though theoretically applicable to massive objects, have vanishingly small results because of the statistical effects of averaging minute random variations.

Test 2. The collision theory is in fundamental contradiction with practically every tenet of mechanics, both classical and modern, to which Velikovsky adds occasional disagreements with other branches of physics. If Velikovsky were right, physics would have to be rebuilt from the ground up. Thus the whole weight of the success of physics in explaining and predicting phenomena is opposed to Velikovsky. Is there a sufficient counterweight to this? The only evidence adduced is a farrago of legends, myths, and opinions. Of these it may be said:

a) It is notorious in psychology, sociology, and anthropology that myths and legends are of dubious evidential value. No matter how many individuals and nations believe in Santa Claus, the serious philosopher or scientist will demand better evidence before accepting the belief, and would never dream of opposing the evidence of legend to that of controlled observation and successful prediction. Even when the same legend appears with many apparently independent origins, as in the case of the "magic flight," we are not tempted to believe it.

b) There are and have been many tribes, many civilizations, and many authors on the surface of this earth, and they have expressed an astronomical number of ideas. Granted the willingness to search through this morass of material, it should not be difficult to collect a large number of legends and opinions favorable to any imaginable hypothesis, and these would exhibit many unexplained coincidences that are perhaps unexplainable except through the operations of the laws of chance.

c) Most of this literary material is further subjected to the author's interpretation before being used as evidence, and these interpretations vary from some that are convincing to some that are absurd.

d) The large mass of unreliable literary evidence used by Velikovsky to prove his point is outweighed by the much larger mass of literary evidence that apparently proves the opposite: cultures whose histories, legends, and traditions make no mention of the events which supposedly happened and that could hardly have been ignored. So Velikovsky proposes a theory of cultural shock, akin to traumatic shock in psychology, which produces a sort of cultural amnesia. The arbitrarily selective nature of Velikovsky's evidence is thus clear, as well as the fact that if cultural amnesia exists, the evidence upon which his case rests is even more unreliable that we have so far painted it.

Test 3. In addition to challenging physics and biology, it is clear that Velikovsky is out of step with astronomy and geology. In minor ways, he is in conflict with chemistry by suggesting that petroleum is produced by the union of carbon and hydrogen in the tenuous material of a comet's tail, or by the combustion of such materials in the atmosphere; and, as suggested before, his views conflict with anthropology and sociology. It even appears from his own description that his position in his own field, history, is equally unorthodox, for he promises another book to contain his reconstruction of history.

Test 4. The collision hypothesis offers no substitute for the challenged laws of motion, nor for challenged laws in other fields of science.

Test 5. Velikovsky's published theories, together

with others not published but hinted at, would appear to present a reasonably consistent view of history. They present, however, nothing that could be called a view, even an inadequate one, of the nature of the universe, physical or otherwise, to replace the sciences with which Velikovsky's theory is incompatible.

Test 6. Velikovsky's theory is in no single instance capable of mathematical accuracy. Its predictions, if capable of any, would certainly be so vague as to be scientifically unverifiable.

Test 7. Velikovsky does show a disposition to accept minority opinions, to quote the opinions of individuals opposed to current views, and even to quote such opinions when they have been discredited to the point that they are no longer held even as minority views. For examples we may cite the notion that the earth's axis has changed considerably, the view of geological catastrophism, the opinions of Deluc, Cuvier, and many of the ancients. Much of Velikovsky's argument is devoted to proving the admitted fact that scientists are fallible and fall far short of a complete understanding of their fields. He is fond of asserting, as an accusation, that such and such scientific views are mere hypotheses, thus showing that he is ignorant not merely of the content of the various sciences, but also of the scientific method itself, and adding the philosophy of science and logic to the list of the disciplines he so rashly challenges.

Accepting as proved the scientists' justification for regarding Velikovsky as a crank, we must still deal with the feeling, first, that scientists should have attempted to refute Velikovsky's position, as a service both to him and to the public, and that they should not have attempted to suppress it, as it is charged that some scientists did. Let us consider the more serious charge first.

Scientists generally regard the publication of articles such as Velikovsky's as unfortunate, since it is misleading to the public and tends to undo the efforts of many scientific educators to popularize science and the scientific method. But it would be surprising if more than a small minority felt that they would be justified in trying to suppress it: free speech is essential, not only to democracy, but also to science. Although the individual scientist probably has no desire to keep a crank book from being published, he has a strong interest in not being associated with its publication. He is well aware, unfortunate as the fact may be, that he will be known by the company he keeps. A few of his fellow-scientists, principally those whose fields of specialization are close to his own, will read his work and decide for themselves, but a large number will form their opinions from secondary sources. If an article is published in a scientific journal whose editors exercise their proper discretion, others will rely on the evident favorable judgment of the editors and assume that the article is a good one. If, on the other hand, a man never publishes in journals of the highest repute, the contrary assumption is made. And, as articles in scientific journals are rarely paid for, it is hard to blame the contributor for refusing to publish in journals of poor repute and to be jealous for the continued good name of journals in which his articles have appeared. Scientists are busy men, and in their reading they will naturally choose those journals whose articles are least apt to waste their time: there are frequent cancellations of individual and even institutional subscriptions to journals that have published crank material, since they may be suspected of doing it again, and they will inevitably lose opportunities to acquire good articles. Much the same considerations apply to book publishing: except in the case of scientists of extraordinary renown, the success of scientific books will depend in large measure on the reputation of the publisher. This is particularly true in the case of textbooks: in selecting a new textbook, most instructors will peruse the table of contents to see whether the approach and subject matter are suitable, and will read a few sections to check on readability and general soundness, but will rely on the reputation of the author or publisher that the rest of the text will be reasonably sound. When the name of a publisher no longer serves to guarantee the scientific integrity of the book, the teacher tends to avoid that publisher.

The so-called suppression of an unorthodox work is thus explained. Is it to be condoned? The danger is that valuable work which happens to be against the current trend might be suppressed along with genuinely crank material. But this threat, though one sedulously to be guarded against, does not constitute a present danger. There are some journals whose editors are overly afraid of novelty, and they may occasionally turn down a worth-while communication, but I know of no field without an adequately liberal journal. Book publishing is wide open: any book that will sell, regardless of its merits, is sure to find a publisher.

And now we come to the last objection to the attitude of scientists: should they not point out to Velikovsky and his public where the collision theory breaks down? As the editors of *Harper's* wrote: "A theory so revolutionary ought to be met by careful appraisal rather than by denunciation and boycott. . . . If the Velikovsky theory is untenable,

this is the way, we believe, in which it should be refuted."

The difficulty in acting in accordance with these very rational suggestions lies in the volume of material required to do so. We ought to list, first, the particular scientific laws violated by Velikovsky's hypothesis. This alone would require the cooperation of many scientists, considerable time in preparation, and no doubt an excessive number of printed pages, yet it would be far from enough. Each of these laws should be explained, the evidence for them should be evaluated, and it should be pointed out just how it conflicts with the Velikovsky hypothesis. A fairly complete textbook of physics would be only part of the answer to Velikovsky, and it is therefore not surprising that the scientist does not find the undertaking worth while. He feels that it should be enough to advise interested people to read any textbook of physics or astronomy, and the fact that neither Velikovsky nor the average lay reader of his theories is likely to do so is not enough to make scientists duplicate existing texts for their especial benefit. But enough of generalities: let us see, by examining one particular instance, what a world of explanation is required to meet even an unimportant part of an irresponsible argument.

Let us first set the stage: Velikovsky's theory asserts that the planets Earth, Venus, Mars, and Jupiter moved erratically within the past few thousand years, changed their orbits, and on at least four occasions Mars or Venus "collided" with the earth. "Collision" evidently means a close approach and not necessarily contact. At each collision the orbits of the earth and moon were changed, and consequently the length of the month and year, and on at least one occasion the earth ceased its rotation and subsequently resumed it. Such an epic naturally meets scientific criticism, the most important aspects of which are the following:

1) According to mechanical principles known since Kepler's time, celestial bodies move in closed orbits, traveling over the same path again and again. Consequently, after the last collision, whatever it was, the two planets involved would be left with intersecting orbits. Various known forces could modify such orbits, but as they would take many millions of years to produce an appreciable modification, it follows that there can have been no encounters between major members of the solar system. Some astronomers believe that Pluto was once a satellite of Neptune, and that a close approach of the two satellites produced the present situation. Whether the hypothesis is true or not, it is made tenable by the fact that the orbits of Neptune and Pluto intersect. Those of Venus, Earth, Mars, and Jupiter do not.

2) A close approach of two planetary objects would indeed alter the orbits of one or both of them considerably and would produce great changes in the orbits of any satellites, but it would have only negligible effects on their rotation.

3) A number of coincidences are assumed. It is extremely unlikely that a single close approach would have occurred once in the past few thousand years. That it should have occurred four or more times is absurd. After close approach, each planet is apt to have a highly elliptical orbit; that it should be nearly circular once is unlikely enough: that it should happen to four planets passes belief. Unless the two planets are precisely the same distance from the plane of the ecliptic at the time of closest approach, the new orbits would not be in the plane of the ecliptic at all. That one encounter should maintain the ecliptic would be amazing: that four or more encounters should do so is unthinkable.

4) An encounter that stops rotation could hardly start it again; not even Velikovsky has suggested how this might happen.

5) A stoppage of the earth's rotation would involve catastrophes of a much greater order than those envisaged by Velikovsky or than those that might have happened on the basis of our historical and geological knowledge.

There are many other objections, but none that we need be concerned with here.

Facing these criticisms, Velikovsky replies by four suggestions, or arguments. The first is that the stoppage of rotation may not have been instantaneous, but may have taken half an hour or so. This is no answer to the first four points, and is an inadequate one to the fifth. His second suggestion is that instead of stopping the earth's rotation, the "collision" may have "tilted the terrestrial axis." This is scarcely less implausible on physical grounds, and does not even agree with the historical "evidence" on which Velikovsky bases his views. The third suggestion is that rotation may have been stopped by the earth's entering a thick cloud of dust. A cloud of dust would retard rotation by increasing the earth's mass, and could have a perceptible effect only by burying earlier civilizations under hundreds of miles of new soil. Finally we come to the suggestion that magnetic or electrostatic forces are responsible for the hypothetical phenomena, and it is this suggestion that we have chosen to deal with in particular. A scientist considers this suggestion ridiculous: let us see how

much demonstration would be required to justify this unfavorable evaluation.

The suggestion that we are to examine has two facets: first, that the sun and planets have important electrostatic charges and magnetic fields, and, second, that these fields would have the results attributed to them. In each case we shall have to consider both electrostatic and magnetic forces, the evidence for Velikovsky's theory, and the theories and evidence of contemporary science.

1) To meet the suggestion that the sun and planets have considerable electrostatic charges, we should first explain what these charges are, and indicate that a negative charge consists of an excess of electrons over protons, and a positive charge of the reverse. We should have to explain that a positive and a negative charge attract and neutralize each other, and that ordinary matter thus consists of approximately equal numbers of opposite charges. We should explain that it takes energy to separate ordinary matter into its constituent charges, and that, unless there is a continued flow of adequate energy or isolation in space, these constituents will recombine. As a result large electrostatic charges are possible in highly dispersed matter such as galactic nebalae, comets' tails, coronae, and stellar prominences; and a smaller electrostatic charge is reasonable for massive hot bodies such as the sun, but large cold bodies will necessarily be close to electrostatic neutrality. We would review the evidence for this understanding of the nature of electrostatic charges, pointing out its success in interpreting and predicting events. We would show that its conclusions about large charges on solid objects are supported by the observed difficulty in handling high voltages. We would point out that charges on large objects are concentrated on their surfaces, that even relatively small charges can be detected with an electroscope, and that the earth's surface is not charged. We would have to deal with local and temporary exceptions (thunderstorms), and show that these are astronomically insignificant and do not alter the over-all neutral status of the earth.

We would then turn to see what evidence Velikovsky offers that planets are highly charged, and find that it apparently consists of accusations that scientists do not understand "the behavior of cometary tails, the movement of solar perturbances, and the round shape of the sun," and have engaged in a conspiracy of avoidance of these topics. We would have to point out that the connection between the premise of scientific ignorance and a particular conclusion is always bad logic, and that in this case the irrelevance of the facts cited to the electrostatic charge of planets is evident. We would no doubt feel impelled to defend the scientists thus attacked, to show that there was no conspiracy of silence, and that in these cases, as in all others, scientists have achieved a great and ever-growing understanding of the phenomena in question, and that in these cases as in all others, there is much yet to be learned. In doing this we should no doubt show why there were unexpected difficulties in measuring the sun's shape, and expected difficulties in determining motions, charges, and fields on the sun's surface, and document the great progress that has been made in these matters. Finally, we should have to comment on Velikovsky's reference to textbooks which say that the earth may have an electrical potential which cannot be dismissed in problems involving the electrical state of heavenly bodies. The possibility—even overwhelming certainty—that the earth is not absolutely neutral is here quoted in an inapposite context. The slight charges, over-all or localized in the ionosphere, are important in affecting the path of electrons, or of other minute and highly charged bodies, and thus enter into the calculations of astronomers and meteorologists, but are entirely negligible in the relations of massive bodies, since the effects are determined not by charge alone, but by the ratio of charge to mass. If a charge is so slight that it cannot move two pieces of silver foil in contact with each other, what effect can it have on astronomical bodies at a nonnegligible distance?

The next item on our agenda would be to consider the existence of powerful planetary magnetic fields. First we should explain the relation between an electrostatic and a magnetic field and justify our theory as before. We would show that where large electrostatic charges exist, as in sunspots, magnetic fields result from motion, and that the magnitude of the field is dependent upon the rate of motion and the intensity of the electrostatic field. We would conclude that since planets have negligible electrostatic charges they necessarily have negligible electromagnetic fields. We would go into the theory of permanent magnets, pointing out that they are necessarily weak and that planets are necessarily weak even as permanent magnets go. This would be confirmed by reference to the earth's own magnetic field and spectroscopic observation of other planets.

We would turn next to the effects of a strong planetary electrostatic charge on the behavior of planets. We would show that objects moving under electrostatic forces, or electrostatic and gravitational forces combined, would obey the same laws of motion as those acting under gravitational forces

alone. This is not only a matter of theory: the successful and accurate prediction of astronomical events must either prove this or prove the absence of electromagnetic forces. Therefore all the objections to Velikovsky's hypothesis would remain, except for a minute change in the second. Electrostatic attraction or repulsion would increase the amount of tidal friction, but not enough to make Velikovsky's hypothesis measurably less absurd. We would point out that electrostatic charges are incapable of applying torque to change the earth's axis.

A peculiarity of magnetic fields is the rapidity with which they fall away with distance. Consequently any magnetic field whatsoever is negligible at a distance and would not affect any but the second objection. Here it would indeed be effective and, provided we postulate a sufficiently strong magnetic field and a sufficiently close approach, could alter the inclination of the axes of both bodies. We might go on to show that an approach close enough to do that would also cause the collision, evaporation, and amalgamation of the two bodies.

The amount of material required to deal adequately with an extremely small part of the total that ought to be dealt with is thus shown to be so enormous that we may appreciate the scientist's reluctance to undertake the task. His duty to educate the public does not ordinarily trouble him: many books and many courses are available for just this purpose. Nor is he much troubled by his duty to educate the crank, for he has learned that the crank is not educable. Most cranks exhibit an inferiority complex, many have the pathological conviction that they are victims of a conspiracy, and consequently there is usually an emotional rather than a rational response to an attempt to educate them. This writer once held a position which brought him many crank letters: against the advice of more experienced men he entered into correspondence with two persons who seemed unusually intelligent. The result was time and effort wasted. By embedding the advice to study further in a highly eulogistic framework, he did succeed so far that one crank replied that the writer was the first open-minded man he had encountered in many years of letter writing. But, alas! on rereading, the crank reversed his opinion.

We have no reason to believe that Mr. Velikovsky will be unwilling or unable to learn: we are merely inquiring into one of the reasons why scientists tend to be skeptical of the value of detailed discussion with one whom they have cause to regard as a crank. Velikovsky may be an exception to the general rule, and it is certainly to be hoped that he will be so. He is a highly intelligent, scholarly, and able man; his facility in writing makes him delightfully readable, and suggests a third reason why scientists may hesitate to cross pens with him. Even the present critic finds him convincing whenever the material dealt with lies in a field in which he is ignorant, and looks forward to a time when Velikovsky's great talents may be devoted to some more worthy enterprise.

ATOMIC WAR

When it is over and the cities lie
With shattered beams across the deadly grooves,
When the last bombs have fallen from the sky,
And nothing lives below since nothing moves,
Surely some scheme of life must rise somewhere
To gauge the elements and lift a crust
That may build up a dream of touch and air—
Love quickening in the straight and crumpled dust.

For I cannot believe that love will fade—
Give up the fight that is as old as rain.
Beyond the Geiger counters we have made,
Something will struggle and be born again.
And there will be a peace beyond all wars
Under the curious language of new stars.

DANIEL SMYTHE

Thoreau: Pioneer Ecologist and Conservationist

PHILIP and KATHRYN WHITFORD

Philip Whitford received his Ph.D. in ecology from the University of Wisconsin in 1948. In the course of the field work for his dissertation he found himself counting stump rings at the same time that his wife was reading Thoreau's Journals *and Burroughs' criticism of Thoreau's science for her graduate work in American literature. After that, interest in Thoreau as an ecologist was inevitable. Dr. Whitford worked in conservation education in Maryland and is now teaching botany and ecology at the Milwaukee extension of the University of Wisconsin. Mrs. Whitford is busy, too—raising two small children and doing research in the trancendental period in American literature.*

DESPITE the scathing remarks of Lowell in regard to Thoreau's nature studies, of Fanny Hardy Eckstrom in regard to his woodcraft, and of Bradford Torrey in regard to his ornithology, modern scientists have gradually come to claim Thoreau as one of themselves. It is our purpose to examine Thoreau's right to the title of plant ecologist and conservationist, and it is therefore necessary first to determine, if possible, Thoreau's relationship to the plant science of his own day.

Modern taxonomy dates from the time of Linneaus' use of the "natural system" of classification. This system was modified by the concept of development, or evolution, into the modern phylogenetic classification, a modification which was taking place during Thoreau's lifetime under the leadership of such men as De Candolle, Torrey, Darwin, and Gray. It is important to note that Thoreau allied himself with these men by his acceptance of the theory of development.

Plant geography was the nearest approach in Thoreau's time to the science of ecology. The exploratory and taxonomic work of such men as Bartram, in America, and Humboldt in South America, was being consolidated in studies that attempted to define the boundaries of the distributions of the world's flora and to determine the conditions and limitations of plant migration. Within Thoreau's lifetime the controversy over migration or specific creation remained unsettled unless one accepts Darwin's *Origin of Species* as the agent of immediate settlement. Thoreau died in 1862 without having read *The Origin of Species.* In this controversy Thoreau rejected the hypothesis of special creation.

Conservation, in Thoreau's day, was not a science, but merely the dream of a few farsighted men and the passion of a group of sentimentalists allied to men like Alcott who were vegetarians because they could not bear to be charged with the destruction of animal life. Ecology as a science was completely unheard of in Thoreau's lifetime, although some taxonomists and plant geographers were contributing work on habitats and associations that served to place them among the precursors of ecology. To this group Thoreau unofficially belonged.

Although the ferment of evolution and specific creation had a place in almost all thinking, scientific or ecclesiastical, in Thoreau's day, Thoreau seems to have taken little part in it. He mentions Darwin several times, but always in connection with the *Voyage of a Naturalist around the World.* There are no references to Lamarck's earlier studies on acquired characteristics. It seems probable that he was somewhat acquainted with the *Vestiges of Creation* even if only at second hand, for Alcott criticized it to a group at Cambridge in 1854, and in a diary entry a few days earlier records that he and Thoreau sat up talking about "genesis" —a term Alcott used in this pre-Darwin period to designate the concept of change. Thoreau's reading of geology, however, would have introduced him to the controversy, though probably his strongest link with the conflict in this country was Louis Agassiz, for whom he took fish and turtles in the Concord River and Walden Pond. The correspondence with James Elliot Cabot, who was cooperating with Agassiz in his work on American fishes, indicates that Thoreau sent specimens during 1847, many of them at Agassiz's request, and that

some of them, at least, had previously been unidentified. In addition to the correspondence about fish, which was conducted largely with Cabot, Thoreau met Agassiz at Concord, where Agassiz came "more than once" and examined turtles with him.

The possible influence of Agassiz cannot be overlooked in any attempt to account for Thoreau's acceptance of the developmental point of view in natural history, for though Agassiz was an advocate of special creation to account for the geographic distribution of species, his work in morphology was based on the concept of development. It would be strange if the subject had not arisen when the two men examined turtles together, for Cabot had written: "The snapping turtles are very interesting to him [Agassiz] as forming a transition from the turtle proper to the alligator and crocodile."

Certainly by October 1860, when Thoreau was puzzling over how new lilies had become established in a small pool in Beck Stow's swamp, he had accepted the theory of development almost without reservation:

> I think that we are warranted only in supposing that the former [the new pool] was stocked in the same way as the latter and that there was not a sudden new creation, —at least since the first: yet I have no doubt that peculiarities more or less considerable have thus been gradually produced in the lilies thus planted in various pools, in consequence of these various conditions, though they all came originally from one seed.
>
> We find ourselves in a world that is already planted, but which is also still being planted as at first. We say of some plants that they grow in wet places and of others that they grow in desert places. The truth is that their seeds are scattered almost everywhere, but here only do they succeed. Unless you can show me the pool where the lily was created, I shall believe that the oldest fossil lilies which the geologist has detected . . . originated in that locality in a similar manner to these of Beck Stow's. . . .
>
> The development theory implies a greater vital force in nature, because it is more flexible and accommodating, and equivalent to a sort of constant new creation. (*Journal*, **14**, 145 [1906]).

The evidence of the above paragraph cited from Thoreau also goes far to prove that, at a time when even Asa Gray was employing a teleological approach, Thoreau was capable of analyzing plant migration in terms that contain no teleological implication, so that the later Thoreau sometimes approached the most modern ecological thought in this respect. That this was not always the case is vividly illustrated by W. L. McAtee's article entitled "Adaptationist Naïveté" (*Sci. Monthly*, 48, 253 [1939]), in which he uses Thoreau as an example of the teleological thought implicit in much of the doctrine of protective adaptation. But Thoreau kept excellent scientific company even in his lapses, for McAtee goes on to say, "It is of interest that the suggestions here quoted and numerous others in his *Journal* considerably antedate those of Belt, Wallace, and Darwin—authors usually regarded as the founders of the adaptationist school."

Passing from the ideological basis of Thoreau's ecology to a consideration of his more specific interests, it is apparent that after the period in which Thoreau was primarily interested in the transcendental implications of nature, there is a long period which may be denoted as a time of orientation, or survey. During this time he devoted himself to identifying plants and noting their habitat groups. Bradford Torrey observes that the *Journals* are the record of Thoreau's study, not the product of an accomplished naturalist, and that even though he was more successful in recognizing and naming plants than birds, there is an abundance of question marks sprinkled after the plant denominations. This is quite true, but it is to be noted to Thoreau's credit as a scientist that he placed such question marks after species of which he could not be certain; and it is also to be noted that the largest number of such question marks appear after the names of grasses, sedges, and rushes, after which many a modern ecologist who is not working specifically with those families is content to write "sp.," meaning that the species was not identified. The fact of the matter is that many members of these families can be identified even today only after careful examination under a microscope, and then only when fruits are present. Thoreau's question marks in many cases are thus a mark of his integrity rather than an indication that he was not adept at identification.

The most readily observed scientific efforts of Thoreau during this preliminary period are his phenological records. Although such observations are common among people who live close to the land, Thoreau was the first man in this country to keep extended records of his observations, a fact that led Aldo Leopold to refer to him as the "father of American phenology." From the phenological lists which occupy much of the space in the early *Journals* and are that part of Thoreau's nature study most frequently reproduced in books for nature enthusiasts, Thoreau entered the realm of the ecologist proper by adding to his phenological data his lists of habitat groups. The combining of the two interests is illustrated vividly in such paragraphs as the following:

> The epigaea is not quite out. The earliest peculiarly woodland herbaceous flowers are epigaea, anemone,

thalictrum, and—by the first of May, Viola pedata. These grow quite in the woods amid dry leaves, nor do they depend so much on water as the very earliest flowers.

This paragraph serves also to remind one of Francis Allen's caution not to assume that Thoreau wrote everything that he knew in his *Journal.* He obviously did not. There are frequent sentences in the *Journals* that have no reference to preceding entries but which refer to questions in Thoreau's mind. Even when all the information is present in the *Journal* it is often present in fragments; yet there is evidence that there was a synthesis in Thoreau's mind. Thus in the excerpt just quoted there is a synthesis of Thoreau's early observation of the growth of certain *Carex* species, skunk cabbage, and marsh marigold in and around springs, with his observation of woodland species.

Like a modern ecologist Thoreau was as much interested in the plants characteristic of certain locations as in rare or atypical plants; and it was these characteristic plants that were usually distinguished and noted on his walks. The *Journals* are full of such entries as this one:

On the first, or westerly, part of the Great Meadows, i.e., the firmer parts and the bank, I find, mixed with sedges of different kinds, much red top (coloring the surface extensively), fowl-meadow (just begun to bloom and of a purplish lead-color, taller than the red-top), the slender purple-spiked panic, Agrostis (perennans? or scabra??). In the wet, or main part, besides various other sedges,—as Carex stellulata, lanuginosa, stricta, etc. etc., —wool-grass, now in flower, a sedge (apparently Campullacea var. urticulata toward Holbrook's) thicker culmed than wool-grass, but softer and not round, with fertile spikes often three inches long, and slender. A great part of the meadow is covered with, I think, either this or wool-grass (not in flower). I am not certain which prevails, but I think wool-grass. . . .

The most extensive of such entries are the pages in the appendix of *The Maine Woods,* in which he listed the typical flora of the woods, river and lake shores, the water, and swamps, together with lists of the typical understory of the woods and the shrubs and small trees of the swamps and shores. These lists are uniformly composed of the dominant species of those sites. The next section of the appendix, however, is devoted simply to lists of the species which "I noticed in the Maine woods, in the years 1853 and 1857."

Shorter, but otherwise similar, habitat groups are recorded on his trip along Cape Cod, but the sand made Cape vegetation sparse, and the lists are short enough to be included in the body of the work. The lists from Mount Monadnock are interesting in comparison with those from the Maine woods, because here Thoreau is concerned with the flora of the mountaintop. In many ways this flora was even less familiar to him than that of Maine and seemingly for that reason his comparisons are sometimes more fully expressed, and he makes the attempt to list the plants in "the order of commonness." The chief difference in his approach to such abundance lists and that of a modern ecologist is that Thoreau's is subjective; one tree composition count in Conantum Swamp and one quadrat count of *Houstonia* are his closest approaches to the objective method. Thoreau's most fully stated comparison of sites occurs here also, together with his partial explanation of why similar species should grow in such widely differing sites as a swamp and a mountaintop.

Though there is little or no soil upon the rocks, owing apparently to the coolness, if not moisture, you have rather the vegetation of a swamp than that of sterile rocky ground below. For example, of the six prevailing trees and shrubs—low blueberry, black spruce, lambkill, black choke-berry, wild holly, and Viburnum nudum—all but the first are characteristic of swampy and low ground, to say nothing of the commonness of wet mosses, the two species of cotton-grass, and some other plants of the swamps and meadow.

Such paragraphs go far to prove that Thoreau's interest lay not in the lists alone, but in explanations that would reconcile the lists.

Despite the conspicuous place which phenological and habitat lists hold in the *Journals* of the middle and early years, they were partially displaced in the last three volumes of the *Journal* by Thoreau's preoccupation with the springs and rivers of the Concord region, and his growing concern for the Concord woodlots. Temperature records of the springs and rivers, observations of water plants, of currents and cuttings of the rivers, of high water and low, occupy many pages of Volumes XII and XIII, in spite of the fact that in editing Thoreau's writings Bradford Torrey omitted most of Thoreau's study of the physiography of the Concord River. It is unfortunate, of course, for the purposes of this discussion that the complete data are not available, for from that given it is not always possible to trace Thoreau's purpose; yet it was from the partial evidence available that E. S. Deevey, Jr., concluded that Thoreau had made an independent discovery of the stratification of water, and made the correct deduction that such a stratification would affect the distribution of fish.

Inspired by his farmer neighbors, who kept asking him to explain why when they cut off a stand of hardwood in a woodlot it was replaced by pitch pine, and when they cut a pine stand the same ground came up to hardwoods as a rule, Thoreau began his intensive study of the woodlots of the

Concord region. Much of the background material for such a study had already been gathered in his years of daily walks in all the Concord region, and in his analysis of tree growth he could draw upon this reserve of knowledge of the region's history and habitat differences. He knew, for instance, that the huge chestnuts he observed in some of the woods and along pastures were probably relicts of the more ancient woods of Concord, and he made lists of the sites that seemed most favorable to the common tree species. Then he turned his attention to seeding, since he did not believe, as many of his contemporaries did, in spontaneous generation or in special creations to account for the distribution of species in the world.

I suppose that most have seen—at any rate I can show them—English cherry trees, so called, coming up not uncommonly in our woods and under favorable circumstances becoming full-grown trees. Now I think they will not pretend that they came up there in the same manner before this country was discovered by the whites. But if cherry trees come up by spontaneous generation, why should they not have sprung up there in that way a thousand years ago as well as now?

If the pine seed is spontaneously generated why is it not produced in the Old World as well as in America? I have no doubt that it can be raised from seed in corresponding situations there, and that it will seem to spring up just as mysteriously there as it does here. Yet if it will grow so *after* the seed has been carried thither, why should it not before, if the seed is unnecessary to its production?

The above-mentioned cherry trees come up, though they are comparatively few, just like the red cherry, and no doubt the same persons would consider them spontaneously generated. But why did nature defer raising that species here by spontaneous generation until we had raised it from stones? (*Journal,* **14**, 333 [1906]).

Thoreau also rejected the theory that seeds lie in the ground for many years awaiting an opportunity favorable for their germination and growth. This left two obvious methods of reproduction—stump sprouts around recently cut trees and seed of comparatively recent origin. He began to observe sproutings from stumps and came to the conclusion that he could sometimes detect a wood which had been cut three times by the positions of trees which were obviously old stump sprouts.

Thus I can easily find in countless numbers in our forests, frequently in the third succession, the stumps of oaks which were cut near the end of the last century. Perhaps I can recover thus generally the oak woods of the beginning of the last century, if the land has remained in woodland. I have an advantage over the geologists for I can not only detect the order of events but the time during which they elapsed, by counting the rings on the stumps. Thus you can unroll the rotten papyrus on which the history of the Concord forest is written (**14**, 152).

Of course, the gradual manner in which many woodlots are cut—often only thinned out—must affect the truth of my statements in numerous instances. The regularity of the succession will be interfered with and what is true of one end of a lot will not be true of the other (**14**, 152).

This concern with stumps and stump sprouts, in conjunction with his knowledge of the chestnuts, seems for a time to have led him to contemplate the further problem of whether he could not "recover" the woods of Concord—that is, work out the history of the woods he was observing and possibly map them by his knowledge of growth methods and the successional pattern of the region. This was one of the plans of which there are frequent hints in the last volume of the *Journal,* but his early death left him no opportunity to expand the design. His observation of stump sprouts, however, led him to the conclusion that, though a woodlot reforested by stump sprouts was a faster and easier source of wood for the farmer than one produced from seed, in the long run the forest produced on sproutland was inferior to the one that came from seed; for the new trees were more subject to the diseases of old trees. He wondered, indeed, whether the vitality of the stock were not eventually vitiated; for in inspecting a woods which he believed had been cut for the third time and each previous time allowed to come up to sproutland he could find almost no new stump sprouts. This lack of sprouts on old sproutland did not explain the succession he was concerned with, but he noted in his *Journal* that for his own part he would prefer a woodlot which had its origin in seed rather than sprouts. He concluded that woodlot management could be improved in Concord by a study of the history of the woods concerned.

Thoreau was, of course, familiar with the practice of crop rotation in agriculture, and he never entirely dismissed from his theories a belief that succession represented a crop rotation in nature—that once the seed of white or pitch pine, for instance, became established on land from which the hardwoods had been cut these new trees throve better than would hardwoods because the "soil was new to them." But the mechanism of succession still evaded him, and he turned his attention to the seeds themselves. He gathered acorns from the white, scarlet, and black oaks of the region and placed them in a drawer for six months or so and then examined them again. Almost none were firm and viable. He observed the results of an acorn blight and the periodicity of pine seeding. Obviously one factor which would determine the growth of seedlings on newly cleared land was what type of seed had been abundant and viable in the season of cutting, but he was aware of other

factors as well, as his observation of maple seedlings attests.

> I see where a great many little red maples have sprung up in a potato field, apparently since the last plowing or cultivating this year. They extend thickly as much as 11 rods in a north west direction from a small tree, the only red maple in that neighborhood. It is evidently owing to the land having been cultivated this year that the seed vegetated there; otherwise there would now be no evidence that any such seeds had fallen here. Last year and for many years it has been a pasture. It is evident that land may be kept in pasture and covered with grass any number of years, and though there are maples adjacent to it, none of the seed will catch in it; but at last it is plowed, and this year the seed which falls on it germinates, and if it chances not to be plowed again, and the cattle are kept out, you soon have a maple wood there. So of other light seeded trees (**14**, 71).

The distribution of pine seeds like that of maples presented no problem. Such seeds were light enough to be carried by the wind, and he concluded that they were everywhere available in the Concord area. The seeds of hardwoods were a different matter. Either he had to account for their distribution or accept the theory that acorns actually did lie dormant in the ground, as many of his neighbors claimed, for fifty or a hundred years —or perhaps longer than that. Squirrels, of course, carried nuts and so did other animals; but did they carry away enough in a single season and then leave them uneaten to account for the seeding of an entire wood? It seemed unlikely—even though Thoreau reasoned that such animals would tend to carry the nuts to a pine woods, which provided greater protection from the winter wind than did the woods in which they grew. This at best was but a partial solution. He extended his observations to include seedlings as well as seed and here made a discovery that satisfied him. He pulled up a small oak seedling one day in walking through a pine lot. Its root was heavy and thick with the growth of several years. The next day he went to all the neighboring pine lots and pulled oak seedlings in each one. Almost invariably they had the fusiform roots of several years' growth—sometimes of as much as seven years. The pine seedlings had no such roots. He then went to oak woods and looked for pine seedlings but he found only very young ones; and then the picture began to clarify. If a pine wood were cut the little oaks which had survived for several years in their shelter were released to the light and had several years' head start over pine seedlings of the year of cutting. Thus the likelihood was that the resultant cutting would come up to hardwoods. In the case of hardwoods being cut the more abundant seed of the pines was likely to produce a pine stand. That explanation, in brief, was what he presented to the Middlesex Agricultural Association as his solution, but his *Journal* supplies the obvious conditioning influences and makes readers aware that Thoreau had not ignored these further factors. If a farmer cut a pine woods and then burned the land, he destroyed the little oaks. Thus in all likelihood he would get a pine stand. Thoreau thought he would do better to let the oaks come up. The "land was pine sick."

Interested as he was in cutover land and the occurrence of stump sprouts, Thoreau's next step was almost inevitable. He became interested in stump rings; and since it should never be forgotten that Thoreau was a practical Yankee before he became a transcendentalist it follows that his interest in stump rings, like his interest in succession, was stimulated by his concern for proper woodlot management. At what age, he asked now, is it most profitable to cut trees? He turned to his measurements of pine stumps and analyzed them. He concluded that they grew

> . . . from first to last about a fifteenth of an inch a year. But they grew very slowly indeed for the last fifty or more years. They did nearly half (?) of their growing in the first third of their existence. . . . I should say that they averaged one thirty-sixth part of an inch the third or last fifty years. That is, their rate of growth the three successive periods of fifty years diminishes in geometrical progression, the quotient being two. . . .
> . . . but I have not yet taken into account the fact that, though the thickness of the layer is less, its superficies, or extent is greater, as the diameter of the tree increases. Let us compare the three portions of the wood. If the diameter at the end of the first fifty years is four, the second fifty, six, and the third, seven, then the amount of wood added each term will be (to omit very minute fractions) twelve and a half, fifteen and a half, and ten, respectively. So that, though in the second fifty the rings are twice as near together, yet considerably more wood is produced than in the first, but in the third fifty the tree is evidently enfeebled, and it probably is not profitable (so far as bulk is concerned) to let it grow any more (**14**, 205).

The stump-ring counts and analysis were in progress at the time of Thoreau's death. In fact, he caught the lingering cold which hastened the family malady, tuberculosis, by kneeling in a wet snow to count stump rings. These notes were not among those which Thoreau spent the final months of his life arranging for publication—probably because he knew no source which would publish them, whereas for such essays as "Walking" and "Winter Apples" there was already a market. Therefore this material remained buried in the *Journals* until 1906 when the *Journals* were published nearly in full. For the same reason Thoreau's often bitter indictment of the manner in which woods and woodlots were managed remained un-

published. As has already been pointed out, there was no public interest in conservation during Thoreau's lifetime. He belonged to the period of the great despoilers of land, not its conservators; yet his own preoccupation with the little world of Concord bestowed a value upon that land which was commensurate with present estimates of land value.

Thoreau wondered, though casually, whether more nutriment was not washed off the brow of a cultivated hill than could be replaced by manuring, but his chief concern was reserved for the woodlots.

> The time will soon come, if it has not already, when we shall have to take special pains to secure and encourage the growth of white oaks, as we already must that of chestnuts for the most part. These oaks will become so scattered that there will not be seed enough to seed the ground rapidly and completely (**14**, 133).
>
> What shall we say to the management that halts between two courses,—does neither this nor that, but botches both? I see many a pasture on which the pitch or white pines are spreading, where the bush-whack is from time to time used with a show of vigor, and I despair of my trees,—I say mine, for the farmer evidently does not mean they shall be his,—and yet this questionable work is so poorly done that those very fields grow steadily greener and more forestlike from year to year in spite of cows and bush-whack, till at length the farmer gives up the contest from sheer weariness, and finds himself the owner of a wood-lot. Now whether wood-lots or pastures are most profitable for him I will not undertake to say, but I am certain that a wood-lot and a pasture combined is not profitable (**14**, 145).
>
> The history of a wood-lot is often, if not commonly, here, a history of cross purposes,—of steady and consistent endeavor on the part of Nature, of interference and blundering with a glimmering of intelligence at the eleventh hour on the part of the proprietor. The proprietor of a wood-lot commonly treats Nature as an Irishman does a horse,—by standing in front of him and beating him in the face all the way across a field (**14**, 132).
>
> We are so accustomed to see another forest spring up immediately as a matter of course, whether from the stump or from seed, when a forest is cut down, never troubling about the succession, that we hardly associate the seed with the tree, and do not anticipate the time when this regular succession will cease and we shall be obliged to plant, as they do in all old countries. The planters of Europe must have a very different, a much correcter, notion of the value of the seed of forest trees than we (**14**, 70).

Thus the charge that Thoreau "tried to see with the inside of his eye too often" loses weight when it is set against the evidence of his researches, and Thoreau is seen as a naturalist who, in a day that was predominantly interested in the discovery and naming of species, was making an effort to synthesize and apply his knowledge of one small area of the country. The lack of statistical analysis, which is the quality a modern ecologist would criticize first, was a lack shared by all biological sciences in Thoreau's day. It is probably this lack that has caused Thoreau to be accepted slowly as a scientist, for, though his work was done before 1862, much of it was not known until the *Journals* were published in 1906, and by that time the concept of science had undergone the radical changes which brought increasing emphasis upon statistical analysis of vegetation. It is interesting to note, however, that the men of literature who as a group have made the most exhaustive studies of Thoreau have been the last to accept him as a man of science. He has been claimed as one of the first conservationists; he has been claimed as a limnologist, and as the "father of American phenology;" his essay on the "Succession of Forest Trees" has been accorded a place in the *Bibliography of North American Forestry*, but only Raymond Adams and Joseph Wood Krutch in the field of American literature have applied to him the term "ecologist," and then only in the most general terms.

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Man in America: A Criticism of Scientific Thought

GEORGE F. CARTER

Born and trained in California, Dr. Carter moved into the East in 1942, initially to serve as an analyst in the Latin American Division of the OSS, but soon thereafter (1943) to teach geography at The Johns Hopkins University. At least one of his Western interests came with him—a curiosity about the origins and antiquity of man in North America. His article is based on a much more solid background of research than mere skepticism regarding the dicta of certain experts.

THERE is a tendency to glorify as scientific a limited and unimaginative style or fashion of thinking. Like many other folkways it has been immortalized in stories. The best one known to me concerns the Layman and the Scientist who shared a seat on a train crossing Nebraska. The Layman looked out the window and exclaimed: "Say, look at those newly shorn sheep." The Scientist scrutinized them carefully and after some deliberation remarked, "Well, those animals do appear to have been shorn on *one* side."

I am dismayed at the dominance of this type of scientific utterance in the field of inquiry into how long man has been in America. Despite the accumulated evidence, we are told that Folsom man and and his peers were the first people to reach America, that they came in toward the end of the last ice age as specialized Arctic hunters following game animals across the top of the world, that all American prehistory, racial, cultural, archaeological, and ethnological, is to be compressed into the subsequent 10,000 years.[1]

Kroeber[2] reports the current point of view as follows:

> In line with the trend of the anticipatory findings that have just been reviewed, it may be said that in the opinion of most Americanists, ethnologists as well as archaeologists, the first human immigrants arrived in the Western Hemisphere in late Pleistocene times. The meagerly known Sandia, Folsom and similar cultures, or rather industries, represent this early level of culture. It is already well differentiated and particularized, enough so to have no exact Eurasiatic counterpart. If anything earlier than Sandia and Folsom existed in America, it has not been found. It is generally assumed that there was nothing notably earlier, because no fossils have ever been discovered in America of Palaeoanthropic men, of Protoanthropic human ancestors, or of anthropoid apes.

Such a statement expressed or implied is commonplace among American archaeologists. To my mind it results from insisting on viewing only one side of the sheep. The devastating thing is that it is done deliberately in the name of science. It seems to rest on a belief that to be scientific one must refuse to "see" both sides of the sheep.

But what does science mean? Does it not mean knowledge? What are we doing as scientists and scholars but seeking to increase knowledge? Are we to give special credit to scientists who insist on wearing blinders, who discipline their minds so that they can discuss one set of facts in isolation from all others? Is it really scientific to state solemnly that only one side of the sheep is shorn?

Folsom culture caught the imagination. The finding of magnificent stone points associated with an extinct bison was one of the turning points in twentieth-century American archaeology. Here was unmistakable evidence for the real antiquity of man in America. Careful study of these and related finds established that this culture flourished toward the end of the Pleistocene period. This became the first widely accepted early culture in America. But now enters "fashion" in scientific thinking. The earliest accepted culture becomes the earliest culture. Whole realms of evidence and logic are ignored. But by emphasizing the established facts (which facts *are* established is again but a fashion of the times), and suppressing the not generally accepted facts, one does the safe, conservative scientific thing. It is overlooked that in so doing one presents a biased and partial truth, and hence less than the truth, the whole truth, so help me as a scientist!

But let's try to examine the question of early man in America. First, we should recognize that like all ideas this one has a history. From 1840 to 1890 it was assumed that man was as early in America as anywhere else. This was a time when geology

and biology were just emerging into the modern period. Geologic time scales and the idea of evolution were just developing. Many errors were made that later could be shown to be ridiculous. Beginning in 1890 the expectable reaction took the form of a school of severe criticism that soon dominated our thinking.

Alex Hrdlicka,[3] on the basis of the erroneous assumption that *Homo sapiens* was a Recent product, proved all skeletal materials found under whatever conditions of antiquity to be not over 5,000 or 10,000* years old because they were *Homo sapiens*. By this Hrdlicka meant a smooth-browed, high-domed skeletal type as distinct from types with massive brow ridges and low vaults. This is a distinction between a Neanderthal and *sapiens* type of man. Hrdlicka could usually match any American skeleton of alleged antiquity with a modern Indian skeleton. To him, this proved the Recency of the allegedly ancient skeleton, and his views prevailed. At the time these views took form, around the turn of the century, they were a fair hypothesis. Today we know they were utterly wrong. The smooth-browed *sapiens* line of men is of parallel antiquity to the heavy-browed Neanderthals. And at Swanscombe in England a modern-type skull, equatable with modern English skulls, has been found in the middle-Pleistocene deposits; hence, *sapiens*-type men were in existence by mid-Pleistocene time and were already assuming racial characteristics. Hrdlicka was wrong in all his fundamental criteria, but his conclusion lives on!

Hrdlicka had a counterpart in W. H. Holmes,[4] whose arguments, reduced to their simplest, ran like this: No implement that can be found on a modern village site can be used as typologic proof for antiquity when found elsewhere. Since everything from battered rocks and simple flakes (eoliths) to the highest forms of flaking can be found on modern sites, there can be no typological proof. The proof of antiquity must, then, be geological and paleontological. But Holmes would accept none of the abundant geological and paleontological proof advanced. Nor would any of his followers. Ergo, there was no ancient man. (I should in fairness insert here that Holmes was seldom as dogmatic about the recency of man in America as was Hrdlicka and was less positive than most of his followers. This, too, seems to be a rule in science.)

Between them, these two men established the pattern that has dominated twentieth-century anthropological thought concerning man in America. It will probably take one or two more generations to change to a more rational view. The beginning of the change is visible, but it is proceeding exceedingly slowly—primarily, it seems, because the bulk of the American archaeologists insist on being scientific, by which you are now aware I mean insist on wearing blinders.

The Nature of the Evidence

But what is the nature of the evidence? What is the core of the problem? What would be a fairly free view of the question of how long man has been in America—free in the sense of uninfluenced by the biases of the past and the dogma of the present? We have quantities of facts, but no one fact that gives an answer so conclusive that it can resolve the doubts we have inherited from the past.

We can begin with the one point on which there is agreement. Since none of man's close relatives is found in the New World and we have no record of pre-*sapiens* man in America, it seems evident that man did not originate in America. This conclusion is reinforced in two ways. We have collected paleontological materials in America for more than 100 years. The absence of pre-*sapiens* men, although negative evidence, is becoming meaningful. Further, in the comparable period, research in the Old World has produced abundant evidence of pre-*sapiens* men. We must conclude, then, that man is an Old World animal who migrated to America after he had reached approximately modern physical status. But here all agreement ends.

Evidences of early man consist of skeletal remains and of artifacts. Typologically early—i.e., simple—artifacts are found in positions suggesting geological antiquity, but so are some of the advanced artifacts such as Folsom points. Which, then, are the older? If typology is not dependable, let's ask the geologist. Geologists, however, are seldom of any help within Pleistocene time. (The late Kirk Bryan, of Harvard, was a brilliant and rare exception.) Further, finds are seldom made under circumstances where landslides, intrusion by burial, etc., *might* not have caused a seeming but actually invalid association. Some of the evidence suggesting the presence of early man is associated with extinct animals once considered to be sure markers of

* It should be emphasized that we know relatively little concerning absolute time. The Pleistocene may have lasted one or two million years. One million years is the most commonly accepted figure. Post-Pleistocene (Recent) time depends on when one considers modern climatic conditions to have begun. This certainly was not less than 10,000 years ago, and may have been 25,000 or more years ago. In the sense that Recent time begins when modern climatic conditions are established, Recent time began at differing times in different places. Greenland is still in the glacial, Pleistocene climate!

Pleistocene time. Indeed some of these animals such as the dire wolf and the sabertooth were once considered as of *mid*-Pleistocene time. Paleontologists (influenced by the anthropologists!) are now unwilling, however, to state when these animals became extinct and consider that some of them could have survived into Recent time. Considerable evidence in the Southwest suggests the presence of man in arid regions when they were much moister than now. But climatic change is very difficult to evaluate. We know that in Pleistocene time ice formed on the continents in higher mid-latitudes and that a series of climatic shifts must have resulted in adjacent regions. But we don't know the exact amount or the geographic range of these changes. Even worse, we don't know the extent of possible climatic changes in Recent (postglacial) time.†

Artifacts are frequently associated with developed soils. But pedologists do not know how long it takes for a soil to develop. A recent text by a prominent soils man presents evidence alleged to show that soils can develop in relatively short time spans. Another has dated soils associated with artifacts as being of an antiquity of 100,000 years. Stone artifacts suggestive of early time because of their lack of development are sometimes greatly altered and discolored. But no one knows how long under natural conditions it takes to alter a siliceous stone, or how long it requires for stones in the desert to acquire "desert varnish." We wallow, then, in an abundance of evidence, but no single line of this evidence is conclusive. We grasp alternately at one and then at another sort, each time vainly hoping that this time we will find *the* technique that will answer our question. Each time we are disappointed. Radioactive carbon studies have recently given promise of dating with some accuracy within the past 15,000–25,000 years. But radioactive carbon dating applies only to some materials, principally wood, which is seldom preserved and only for a limited time. No science or scientist, geologist, climatologist, glaciologist, pollen analyst, pedologist, or chemist, has been able to give us *the* answer.

We lack an expressed methodology for handling this problem. Yet there is one, and its simplicity is so great as to have apparently concealed it. Although no one field gives an absolute answer, each gives a probable answer. When these probable answers all indicate the same thing, this concurrence comes as near to establishing a fact as it is possible to do in some fields, perhaps in any field. Does not science rest upon proofs that are merely higher degrees of probability attained by compounding lower degrees of probability?

An analogy would be a forest in a wind. All the trees in the forest would be bending before the wind. Should you ask why a particular tree is bending, you would have to consider that trees bend for many reasons; that although it is true there is a wind blowing, one must consider slope of the ground, soil creep, early accidents of growth, a tree's tendency to grow toward light, and so on. Hence one could not conclude from observation of this one tree which way the wind is blowing. Yet, obviously, we *can* determine which way the wind is blowing! We must try to see the forest.

Nature of the Evidence: Culture Levels

There were three general levels of culture in America which could well be called Neolithic, upper Paleolithic, and lower Paleolithic. The latest in time is of course the Neolithic agriculture-and-pottery and bow-and-arrow-using level. The time of beginning of agriculture varied in the United States, the earliest known date being about 1,000 B. C. at Bat Cave in New Mexico. The earliest known date does not mean this is the beginning. It is the earliest known date and nothing more. Plantsmen all recognize that a long period must precede the appearance of a domesticated plant.

Preceding the agricultural levels there is a long history of hunting people who used the throwing spear (atlatl), had a wide range of specialized scraping tools, and were skilled makers of stone points. The earliest of these are associated with the extinct animals and climatic distributions differing from those of today. Folsom was one of these cultures. Sandia is somewhat earlier. There is clearly a general culture level in late Pleistocene time when men in America possessed hunting equipment comparable to that possessed by the specialized hunters of Eurasia in the same time period. For convenience I shall refer to this general level as the Folsom period or culture.

Great uncertainty seems to surround the lower paleolithic-like cultures whose remains are widely distributed in the Americas. It has been noted that they are often associated with the early long-headed types of Amerindians and that they survived only in marginal areas. But the tendency is strong to consider them either decadent survivors of the earlier hunting cultures or perhaps later immigrants to America from some Asiatic reservoir of primitive folk. One gets the impression that any imaginable

† Actually, we know more than is usually admitted. As an example, studies such as Luna Leopold's Pleistocene Climate in New Mexico (*Am. J. Science*, 249, 152 [1951]) show that the postglacial pluvial periods, long a stumbling block in our thinking, probably never existed.

explanation is acceptable if it will prevent one from facing the unpleasant probability of great antiquity for these cultures. Here the history of ideas enters again. Books have been written and lectures given: Man is Recent in America. How can we retreat?

Culture Level and Time of Migration: The Upper Paleolithic People

The earliest culture now known in the Bering Strait area was an upper-Paleolithic–level culture resembling the Folsom culture. The Folsom culture entered America sufficiently early to spread its traits widely over North America. Its influence reached at least as far as what is now the Papago Indian territory in the Sonoran Desert of southern Arizona at a time when that land supported not only horses and other Plains animals that would be somewhat unexpected residents there today but a ground squirrel that today lives in open forest country and a tapir whose needs are permanent water and probably at least 20 inches of rainfall.[5] The rainfall in the Papago's desert today is 7 or 8 inches, and there is no permanent water.

The evidence, then, is that the Folsom culture reached the Mexican border at a time when extinct animals flourished in deserts, which were then climatically markedly different from the present. This is, by definition, in pre-Recent times—that is, in Pleistocene time, prior to the disappearance of the great ice masses in the north and the beginning of modern climates. This finding in turn raises a number of problems. How did men survive their arctic journey? How long did it take them to reach Arizona? And, equally important, why did they go there?

The problem of survival of early entrants during their passage through the Arctic is little discussed. In culture level the Folsom people were equivalent to the upper Paleolithic peoples of Europe, who in the latter part of the last ice advance (Wisconsin time) lived in proximity to the continental glaciers. The detailed resemblance of modern Eskimo culture to these upper Paleolithic cultures has often been noted. That the Eskimos, with their tailored fur clothing and stone lamps, are capable of living in the high Arctic wherever there is food is amply demonstrated. That the upper Paleolithic arctic hunters possessed tailored fur clothing comparable to that of the Eskimos is probably not too great an assumption. Indeed, the shoe is on the other foot. What evidence is there that man could survive on the treeless arctic tundra without tailored skin clothing and the stone lamp? If the Folsom level of culture lacked tailored skin clothing, how did its bearers survive the Arctic? Is there an implication of a warmer period?

There are still other factors here that need discussion. First, the Eskimos do not move south; they remain in the environment to which their culture is fitted. However, certain traits—for example, one type of pottery—diffused southward from a Bering Strait, seemingly Eskimo, source. If it is a human tendency to remain in the environment to which one is accustomed, how are we to think of the Folsom people? Were they different? Did they move freely and rapidly from one environment to another? Or did they, like the Eskimos, tend to cling to their place and way of life? Was it perhaps the Folsom techniques of hunting and stone-flaking that actually diffused? This is an important general question. Which moves farther faster—men or ideas? Does not all human experience indicate that it is ideas that travel? But for this to happen there must have been pre-Folsom man in America. (More of this later.)

Further, there is the curious question of food for Folsom man. If he was an arctic hunter, there would be little difficulty in his entering America during a glacial period. Much of Siberia was ice-free, as was adjacent Alaska, and the tundra-ranging reindeer would have drifted across the land bridge at Bering Strait created by the lowering of sea levels that resulted from the withdrawal of water from the sea to form the great ice masses on the continents. Tundra hunters would find the road open and a familiar habitat and economic basis for their society. But what lay to the south of them?

During a glacial period, when the land bridge existed, great sheets of ice covered the land; these would be barren of all food and hundreds of miles across. There would be no incentive to cross such vast wastes. Yet Folsom influences (the man or his ideas or both) did spread south and so early as to reach into the deserts when they were experiencing Pleistocene climates and, by inference, when the great ice masses still existed in the north. Does this mean that the Folsom culture must have been in the American Arctic at least in middle Wisconsin times, so that during one of the retreats of the Wisconsin ice, migrations to the south, or contacts with people to the south of the ice, could take place?

Furthermore, we must take into account more than just Folsom cultures. In Sandia Cave, near Albuquerque, New Mexico, the Folsom layer was sealed off from the modern deposits by a layer of calcium carbonate. Beneath the Folsom layer lay a sterile stratum of yellow clay, and beneath it lay another layer containing dart points. This indicates

the existence of a hunting culture a considerable time prior to the Folsom culture. And the clay and calcium carbonate layers carry undeciphered implications of great climatic changes. Similar indications are to be found in the Mohave Desert on the shores of extinct lakes. Crude blade cultures are followed by simple blade cultures, which are finally replaced by developed blade cultures, among the remains of which at least one Folsomoid point has been found.

On this basis alone it seems that the whole truth is best stated as follows: The upper Paleolithic hunting cultures characterized by specialized flaking techniques such as those of the Folsom culture must have entered America at a time when it was possible to move (or diffuse ideas) southward, and this means at a time when the continental ice mass did not form a barrier. But the evidence also indicates that this occurred at a time prior to the extinction of many Pleistocene animals and prior to the establishment of modern climatic conditions. It would seem that the latest time this southward movement could have occurred would have been in the last Wisconsin interstadial. If there was more than one immigration of upper Paleolithic hunters with a considerable time interval between their comings, as is suggested by Sandia Cave, then the first group may have entered in an even earlier interstadial. This is Pleistocene time as defined paleontologically, climatically, or in terms of glaciation. Why is it, then, usual to qualify the dating of the Folsom level of culture as "late Pleistocene *or early Recent*"? Does not the qualification amount to an unnecessary obscuring of facts?

Evidence for the Antiquity of the Lower Paleolithic People

But this is a mere quibble compared to the next question. Are the upper Paleolithic hunting cultures really the earliest culture levels in America? No one can deny the presence of simpler culture levels. Their remains are abundant. Are these cultures decadent survivors of earlier more cultured folk? This assumption is often made. Or do they represent simpler folk who somehow entered America in a later (that is, Recent) time and filled ecological niches unoccupied by the higher cultures? The evidence seems abundant and clear for the presence of the simpler cultures long prior to the later, developed cultures. I shall review here evidence from field work in the Mohave and Colorado deserts and about San Diego on the adjacent coast. I have worked in these areas intermittently since 1930. The desert work was done under the direction of Malcolm J. Rogers while I was curator of anthropology at the San Diego Museum of Man (1934–38). More recently I have spent my summers studying intensively the coastal geomorphology in relation to the antiquity of man.

In this region, extending from the coast of Southern California inland to the Colorado River, one finds a characteristic series of evidences of man: (1) Signs of man's occupation but no stone tools. (2) Flakes and cores. (3) The grinding slab and stone tools limited to flakes and cores but lacking definite fixed forms. (4) Addition of stone tools of recognizably fixed form—chopping and scraping tools predominant. (5) Blade cultures probably indicating the introduction of specialized hunting. In the desert this sequence can be elaborated. Crude blades appear first, then recognizable dart-point cultures in a long series, the earlier part of which probably correlates with the Folsom culture in time. (6) Arrow-point and pottery-making people who extend to historic times.

Typologically one could argue for a very long pre-Folsom period. But a typological argument could be destroyed if it stood alone. Fortunately, however, it stands in a forest of facts. Stones on stable surfaces in the desert become colored after a lapse of time. This so-called desert varnish, a rich chocolate-brown coloration, coats almost all stable rock surfaces. How long it takes to form and even its exact origin and nature are not known. It marks our archaeological remains most interestingly. Stonework of the arrow-making people never shows a trace of it. Stonework of the very fine dart-point cultures never shows any such coloration. The cruder, smaller dart-point materials sometimes show a faint trace, and the large blade materials often show a slight varnish. The tools typologically earlier than this have varnishes as heavy as any of the natural stones of the desert. The presence on such ancient land surfaces, often in areas remote from any possible water supply, of thousands of stone circles three to six feet in diameter presents an interesting problem. Some of these are but cleared spaces in the desert pavement. Others have rims composed of boulders. The circles are surely the work of man, for there is no natural, as opposed to human, cause for them. We know that ancient man sometimes built low circular windbreaks, put a small fire in the center, and curled up to sleep. A brush shelter with gravels cleared from the center and piled around its base would leave a comparable mark. Yet over large areas containing such circles there are no stone tools of any kind. This suggests a remote time and a culture level lacking any stonework. The stones forming the rims of the circles are as varnished as the surrounding stones,

which have lain undisturbed for geologic time. Is there any way to interpret all this except as an indication that the typological series is also a valid chronological series?

In arid regions the phenomena known as desert pavements generally mark ancient alluvial fans in which the intermittent desert watercourses are now entrenched. When fan surfaces are thus freed from deposition, wind erosion removes sufficient fine materials to leave a layer of rock, a mosaic just one rock thick. These armored land surfaces will persist in a desert for great lengths of time. Even their creation requires much time. And, since we are interested in time, let us study these facts.

No arrow-point or dart-point site in the Colorado or Mohave Desert known to me has been reduced to a desert pavement, in spite of the fact that some of these sites are on gravelly, silty, or sandy materials readily converted to desert pavement. Geomorphology, then, like desert varnish, supports the validity of the typologic sequence.

There is a nice unsolved problem related to the dissected fans of the desert and their varnished surfaces. If these fans are now inactive, subject to almost infinitely slow dissection, under what conditions were they built? Those conditions were certainly different from those of today. They are also remote in time. If the late Pleistocene hunters' campsites have not yet been converted to desert pavement, nor coated with a trace of desert varnish, how much time must be allowed for the development on these fans of the maximum intensity of both conditions? That is, since the 10,000 years or more of post-Folsom time are insufficient even to establish the desert pavement, much less to allow the development of intense desert varnish, which can only occur after the pavement is established, must we not conclude that the artifacts limited to desert pavement areas and coated to the maximum extent with desert varnish are some multiple of 10,000 years of age?

There is another series of natural factors that has already been touched on but that is worth development. The arrow-point sites are always at modern water supplies. This agrees well with their recency, probably not earlier than the time of Christ. The advanced dart-point cultures are associated with conditions somewhat moister than today. The Folsom level of dart-point cultures (San Dieguito or Playa cultures in the Mohave Desert) are associated with now-extinct lakes. The preblade cultures are associated with ancient fossil landforms and are sometimes totally unrelated to present water supplies. There is general agreement, then, of the implement typology series with a climatic series, indicating a change from much moister than now to somewhat moister to modern aridity.

An interesting speculation concerns the absence of the preblade cultures from the lake shores in the Mohave Desert. Why did these people avoid the attractions of a lake shore? To such a people a lake shore would be most attractive. Fish, shellfish (fresh-water mussels), roots and bulbs, bird life, and small mammals abound in such locations. Probably man has always found such situations attractive. The later people, even those who seemingly specialized in hunting, always came to such localities. Can the absence of the preblade culture artifacts means there really *is* no preblade culture? Can it mean the nonblade materials are merely refuse from the later people's tool-making activities (shades of Holmes again!)? Or, can it mean that such materials are merely the product of decadent survivors of the earlier blade cultures?

I think that the evidence from desert varnish shows that none of these negative views is tenable. The climatic evidence also gives a check on the typological sequence. The general picture is one of cultural advancement, not degeneration. What, then, is the meaning of the absence of the preblade tools from these ecologically advantageous sites?

Man would never have passed up so good an ecological situation. But lake shores are geomorphologically active zones. There has been a series of lakes in each basin. The shorelines have fluctuated, and the beach materials have been worked and reworked. An excellent proof of this is to be found near the overflow of ancient Lake Mohave. On one of the highest beaches greatly eroded tools, mostly blades, are to be found. In some instances blades made of porphyry, a very hard stone, are so worn as to be recognizable only by their outline. The type of erosion is identical with that created by surf action on tools made of comparable stone. If the still-earlier people left tools about this and other lakes, they must have been subject to even more erosion. Any that are left in recognizable form must be buried in beach gravels. If sought, they would very likely be found. But, to date, precious little digging has been done in the ancient beaches of the desert lakes. Why dig for crude artifacts when beautiful blades lie on the surface? Why dig when you *know* there is nothing there?

Incidentally, this type of evidence has a bearing on the Sandia finds. The sequence of strata in Sandia Cave suggested climatic changes associated with cultural sequences. Like the sequence of lake levels, the sequence of deposits suggests alternations of arid and humid conditions. Changes of the duration and magnitude indicated would seem to re-

quire interstadial to full glacial shifts. Since in the Mohave Desert and at Sandia Cave two such cycles are indicated, we have a suggestion that we are dealing with much, if not most, of Wisconsin time.

Now to gather our sticks into a bundle. In Table 1 the different lines of evidence are put side by side. It can be seen that these separate phenomena are in mutual agreement. The culture level of the latest material to be found on latest lake shores is relatable to Folsom. Indeed, one Folsomoid point was actually found on one of the terraces. Further, these lakes were not ephemeral features but sufficiently long-lived to build large beach ridges and to cut benches into rock. All this indicates large-scale, long-continued climatic changes. Culture, geomorphology, and climatology are all in agreement that these lakes are Pleistocene features. The sequence of shorelines with cultural materials suggests that we are dealing with all of Wisconsin time.

The flake and core and stone circle materials pose a special problem. There is little or no transition in desert varnish and desert pavement from them to the next culture. Desert varnish goes from faint to intense, desert pavement from trace to intense. The degree of development suggests a time difference of large magnitude (Table 1).

We begin now to run into difficulties with the accepted view of things. If the great hunting cultures with their advanced flaking techniques are of late Wisconsin time (last Wisconsin interstadial at least, perhaps earlier), and if their analogues in the desert are the dart cultures, then where in time do nonblade cultures, desert-varnished artifacts, and stone circles fall? Must they not extend farther back into Wisconsin time, at least? If so, we are presented with pre-upper Paleolithic peoples. That is, we have earlier than late Wisconsin immigrations to America of an either nonlithic people or at best eolithic peoples. The problem of their entry through the Arctic is a very different one from that of the upper Paleolithic peoples. Before attacking that, however, the evidence of comparable cultures from the coast of Southern California should be examined.

Evidence from the Coast

The coastal area about San Diego, California, is a virtual museum of landforms and soils. Throughout Pleistocene time the alternation of high and low sea level has created on this relatively stable coast[6] a series of sea and valley terraces. On these terraces soils have formed. Often there are two or more maturely developed soils one over the other. In this area the same typological sequence found in the adjacent desert occurs: (1) no stonework, (2) simple flakes and cores, (3) developed core tools, (4) blade cultures with refined flake and core tools, and (5) pottery and arrow-point cultures. A lively discussion revolves around the priority of the cultures with simple stone tools. Perhaps they are merely decadent survivals of the blade cultures? Such a view is possible if one wishes to see one side of the sheep, or limit oneself strictly to archaeology.

TABLE 1

Archaeology	Climate	Desert Varnish	Desert Pavement	Cultural Correlation	Chronology
Mohave and Colorado Pottery and arrow	Modern	None	None	Pueblo	A.D. 1000
Darts					R
Amargosa II*	Near modern	"	"		E
Amargosa I	" "	"	"	Basket Maker	C
Pinto-Gypsum	Moister	"	"		E
					N
					T
					10,000
Playa II (San Dieguito)	Lakes, much moister	Trace	Trace	Folsom	P
					L
Playa I	Lakes, much moister	Faint	"		E
					I
					S
					T
Flake and core (Malpais)	Much moister, lakes probable	Intense	Intense		O
					C
Stone circles	Much moister, lakes probable	"	"		E
					N
					E

* Technical names for cultures are from M. J. Rogers *Early Lithic Industries of the Lower Basin of the Colorado River and Adjacent Desert Areas, San Diego Museum Papers*, No. 3 (May 1939). I have not used the revised chronology given in Haury's *Ventana Cave* because its basis has never been explained and seems quite inexplicable to me.

For the great test of archaeology, a clear case of one culture overlying the other, has not been reported in this area. Actually, such sites do exist but have never been published. But, just as in the desert, there are other sources of evidence. In the desert the climatic sequence, but especially the desert-varnish sequence, validates the typology. On the coast, soils and landforms are the independent checks on the typology.

Accumulations of camp refuse, or middens, are numerous along the coast of Southern California. Locally these are several feet thick and full of shellfish remains. They can be ranked in relative age by the degree of alteration of the midden matter. Middens containing pottery and arrow points are black in color and fluffy in texture. Other middens containing a comparable artifact assemblage, but lacking pottery and stone arrow points, are gray in color, have distinctly less shell in the upper layers, and have a distinct accumulation of lime in the lower layers. Another group can be recognized having very little shell left in the upper layers and that upper layer having a brown color little, if any, different from the adjacent natural soils. The lower layer, however, is strongly lime-cemented. Still another group consists of middens that have lost the original color throughout and, most significantly, do not have a lime-enriched deep layer. Instead, they possess a clay horizon and are either lime-poor or even lime-free and acid in reaction. It is this type of midden, if it can even be called such, that contains neither blades nor developed core tools, but only flakes, cores, and grinding slabs.

Anyone acquainted with the concepts of soil formation and the distinctive sorting of materials with the passage of time will recognize in the first part of the series of soils described above, a characteristic sequence. Leaching steadily removes soluble materials and fine particles and carries them downward. The depth to which they are carried depends largely on the permeability of the soil material and the amount of rainfall. The accumulation of lime in a zone about 18 inches thick with its top at a depth of about 18 inches is approximately what one would expect in the present climate. Actually, the degree of removal of soluble materials and their depth of accumulation is, if anything, more than one would expect with the present low rainfall. The sequence from fluffy black to compact gray, with a lime-cemented lower layer, to a brown surface layer with a strongly cemented lower layer, is to be expected in a region on an arid-humid boundary. How much time is required to accomplish the maximum change is a difficult question. There are many variables: parent material, climate, slope, time, and so forth. Most of these factors are uniform within this limited region. Examples of this whole series, all developed from a common parent material (midden matter), can be found on comparable surfaces within a mile of one another. All other factors thus being relatively constant, it is evident that the major factor leading to their becoming recognizably distinct is time. But, how much time?

With V. P. Sokoloff I have been inquiring into the whole problem of soils and time. (Dr. Sokoloff will present a separate study on the general problem, and we will jointly present detailed soils studies.) It is our general conclusion that all of Recent time, 10,000 years or more, is required to give results such as those described for the soils with lime accumulation. But if the soils ending with a leached upper layer and a limy lower layer represent all of Recent time, what is the meaning of the acid soils with clay in their lower horizons?

In terms of our general knowledge of soils, clay-pan soils are markers of humid climates. The presence of acid, clay-pan soils and lime-pan soils side by side is as contradictory in its implications as musk oxen and monkeys appearing side by side. Since the calcareous soils are climatically *in situ*, the acid soils must represent a former time of more humid climates. Climates do not change in localized spots, but shift over broad areas. This suggests that the humid climates that produced lakes in the desert and made now-barren areas livable are to be equated with these acid soils of the coastal region. It should not be surpising, then, to find that the correlations extend to the cultures. The dart-point culture that equates with Folsom, a late Pleistocene culture, is in the Mohave Desert associated with the last persistent stand of lakes at high levels. On the coast it is associated with the upper (A) horizon of acid, clay-pan soils. Its failure to occur beneath such soils suggests that the culture dates to, but does not precede, the period of formation of the last of these soils. Artifacts from the earlier cultures that in the desert are associated with extreme desert varnish and desert pavement are in coastal Southern California found beneath not just one clay-pan soil, but may be found beneath two or more superimposed soils and by rolled artifacts from the ancient valley-fills of the rivers.‡ From detailed study of such features as an alluvial fan at La Jolla we know that acid clay-pan soils formed two or more times during the Wisconsin period (Table 2).

‡ A separate report on these rolled artifacts is in preparation. That these artifacts are of the age of the gravels in which they are found is indicated not only by stratigraphy but also by the attrition they underwent when rolled in the stream bed at the time of their deposition.

That artifacts are found beneath the full series indicates that man was present during all of Wisconsin time. Rolled artifacts from the 60-foot valley-fills of the rivers of this area are evidence of still greater antiquity. They represent man in third interglacial time. This is perhaps the explanation of the discontinuity from traces only of desert varnish and desert pavements to the extreme developments of these features that we mentioned in discussing the desert cultures; that is, the artifacts with extreme coloration associated with maximum desert pavement development may also be of third interglacial age. Actually, it seems more logical to think of them as of third glacial age, for only during the increased humidity of glacial times would the deserts be so inviting and habitable as to allow primitive man to occupy the areas where his house sites are now to be found.

The evidence from the coast and the desert leaves little ground for doubt concerning the priority of the lower Paleolithic level of culture. The upper Paleolithic hunting cultures equipped with the throwing spear (dart or atlatl) are of Wisconsin and Recent time just as they are in the Old World. The lower Paleolithic peoples equipped with extremely simple stonework were present in America during all of Wisconsin time, probably during third interglacial time, and possibly even earlier. We return now to the question of how the lower Paleolithic people reached America.

TABLE 2

COASTAL, SAN DIEGO COUNTY

	Cultures	Soil Types	Climate	Alteration of Stonework	Geomorphology
	Bow and pottery (Diegueno)	Black, fluffy, and undifferentiated in depth	Modern; sites at modern water supplies	None—altered tools of earlier periods resharpened; fresh surfaces unaltered	Sea level high and rising
DEVELOPED	Pre-pottery Diegueno; continuation of San Dieguito but usually in stone—not suitable to fine work	Dark, compact on surface; brown and slightly limy at depth of *ca* 2 feet			
		Brown surface; dark and limy at depth	Approaching modern aridity Sites near modern water supplies	None	Rising sea level; occupation extends to times of somewhat lower sea level than now
	San Dieguito—fine blades and scrapers; much work in alterable stone	Acid soils with clay pans; artifacts never in or below clay pan	Moister than now Site often distant from modern water supplies	Felspathic volcanic rock (felsite, dacite), strongly altered	In valley-fill of San Dieguito River; otherwise on modern surfaces
ARTIFACTS	Flake, core, and metate	Acid soils with clay pans; often two or more superimposed; often show sequences of salinization and desalinization	Moister than now Sites often distant from modern water supplies	Used river gravels that do not alter	Throughout Wisconsin age alluvium
	Flake and core				In 60-foot valley-fills of Sweetwater and San Diego rivers
	No stone tools				Middens without artifacts

Lower Paleolithic Cultures: Arctic Survival

Having now viewed the forest of facts and found that there is good evidence for the priority in time of the lower Paleolithic cultures in America, we must examine the question of how these people reached America. This is what we know about them. They had little stonework, only simple flakes and cores. Nowhere among possible cultural survivals, such as the people of Tierra del Fuego or Lower California, is there any hint that they ever possessed developed shelters and clothing. The simple cultures known archaeologically in the Great Basin, where aridity in caves has preserved perishable materials, present a similar picture.

This is in agreement with what we know of Old World cultures. Man did not live in cold regions until upper Paleolithic time, when he possessed the bone needle for sewing furs (hence, tailored skin clothing?), specialized scraping tools for dressing hides, and good darts and harpoons for hunting.

But the presence of lower Paleolithic-like stone cultures in America during glacial times indicates that at some time people of this culture level managed to survive subarctic climates in the Bering Strait region. Further, they crossed a water gap of some magnitude; or else they crossed at a time when there was a land bridge. A land bridge certainly existed at times, for many animals crossed from America to Asia. When ice accumulated on the land, the sea levels fell. A small lowering of sea level is sufficient to create a land bridge at Bering Strait. But in glacial times of lowered sea level the Arctic should have been colder than it now is! Yet it is most unlikely that anyone could survive in the Bering Strait region today without equipment approaching that of the Eskimo. We seem to face a contradictory and impossible situation!

The Arctic must always have a long, cold winter. This is an inevitable consequence of a round earth whose axis is inclined toward the plane of its ecliptic. Yet from paleontology we know that during Pleistocene time the Alaskan region was warm enough for horses, camels, mastodons, and other animals that are not usually associated with arctic cold, and that these animals crossed from America to Siberia. We have often been misled by these facts, however. Camels are at home in Siberia, as are horses, and mammoths are not simply African elephants, but include cold-adapted species. Yet this should not be overstressed either. The natural range of camels and horses stops far short of the Bering Strait region today. Neither did camels and elephants swim across the Bering Straits. At some time in the Pleistocene there must have been a concurrence of land bridge and somewhat milder than present climate. Lower Paleolithic man could, and probably did, make his American debut at that time. Later, upper Paleolithic man could have crossed at any time, for he can be assumed to have been culturally equipped.

Someone must examine the forest of Alaskan-Siberian facts before we can fully understand how and when this crossing occurred. From the evidence in Southern California it could not have been later than early Wisconsin time, and, if the materials from the alluvial fill of the river valleys of Southern California are indeed artifacts, it must have been as far back as early third interglacial times, or earlier.

Part of the answer will be found to lie in a more geographical consideration of Pleistocene climates. The great climatic shifts accompanying glaciations have been too simply conceived.§ Not all parts of the Arctic experienced the same degree of glaciation at the same time. As a first hypothesis, I would suggest that the onset of glaciation may have been marked by a shift of the great centers of atmospheric circulation (such as the Aleutian low and the Siberian high) in such a way as to bring a stream of warm oceanic air into the Bering Strait region. This air would accomplish several things. It would change the climate significantly toward a milder marine type, especially in the coastal regions. It would increase precipitation and hence lead to the accumulation of snow on the mountains and hence to glaciation. If this were part of a world-wide movement, the increasing snow accumulation on the land would lead to a falling sea level. When only a fraction of the ice accumulation that marked a major glaciation had occurred, the sea level would have fallen sufficiently to create a land bridge at Bering Strait. If the climatic change was great enough to have created a sufficiently mild maritime climate, and *if* the accumulation of glaciers along the Alaskan coast was not great enough to nullify the climatic amelioration or to reach to the coast and hence close the narrow coastal corridor, then for a brief period (a thousand years?) lower Paleolithic men could have trickled across a land bridge, in a time of climates milder than now, and so have entered America. Here my line of reasoning traps me. If the evidence from the deep valley-fills of San Diego County really indicates the presence of man in America in third interglacial time, and if

§ I owe much to J. R. Mather, whose study of probable Pleistocene atmospheric circulation patterns has advanced our thinking concerning Pleistocene climates a long way beyond our former simplistic views. (Pleistocene Climates and their Effect on the New World Migration of Ancient Man. Ph.D. thesis, The Johns Hopkins University [1951].)

the most likely time for his entrance into America is at the onset of a glaciation, then man must have entered American at the beginning of the third (Illinoian) glaciation. Time and work will provide the answer to this question. The possibility should not be ignored even though it is at complete variance with all anthropological theory today.

Now to review the results of this survey. I have presented some new facts but mostly I have combined well-known facts‖ and attempted to interpret the meanings of these combinations. If I have proved anything, it is that there is a lower Paleolithic culture level in America much older than the upper Paleolithic hunting cultures. How old these lower Paleolithic cultures are is still speculative. But until we speculate about what may be the condition of the far side of the sheep, we are unlikely either to bother to look for evidence or to evaluate fairly the evidence produced by those with the temerity to look. My speculations lead me to suggest that it is time we looked again in the Wisconsin and Illinoian glacial deposits and in the last interglacial deposits for evidence of early man.

‖ And called attention to published but little-understood facts, for the gist of the climate, desert varnish, and landform evidence was published by M. J. Rogers (*loc. cit.*). His conclusions, however, were at the opposite pole from those presented here.

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THE MASTODON

(Remains Discovered near Lodi, Ohio)

We delve into the earth; we dig and tear
With aching hands to where, two feet beneath,
Within a shallow grave, half-buried there,
The massive thigh bone, the preponderant teeth—
And fifteen thousand years dissolve away
(Less than an instant reckoned out of time)
While still we struggle through the muck and clay
To lift the past out of the layered slime.

Then we will match the pieces, bone to bone,
Seeking to fit these fragments that remain
Into the common, pristine mastodon
That once ran wild on the postglacial plain.
Yet, as we dig, we pause and dimly sense
We are the fossils of millennia hence.

Mae Winkler Goodman

Something about Flicker-Fusion

CARNEY LANDIS

Dr. Landis is principal research psychologist at the New York State Psychiatric Institute and professor of psychology in Columbia University. He has published widely in the field of abnormal psychology and is the author of a well-known text in this field. His interest in flicker-fusion grew out of the work done in the Columbia-Greystone Brain Research Project, in which he held the title "Responsible Investigator" for the psychological discipline. His article is based on the Walker-Ames Lecture, which he delivered at the University of Washington last May.

WHEN one looks at any source of intermittent illumination, the light may appear to flicker or it may appear to be steady, depending on a variety of circumstances. The point where some change in physical stimulation causes the flickering sensation to appear or the "steady" sensation to change to flicker is called the critical flicker-fusion threshold (CFF). The history of observation of and speculation on CFF is a long and interesting one, which culminates in certain unusual recent developments.

Properly speaking, the study of flicker-fusion began with observations on the persistence of vision. "All the world knows that if in the dark a flaming coal is turned rapidly, one sees a continuous luminous circle as if the coal left in the air a trace of its path." With this statement, Plateau, who became one of the great physicists of his day, began his dissertation for the Doctor of Science degree at the University of Liége in 1829.[1] With this idea, Plateau also invented the Phenakistoscope—the first motion-picture machine—in 1829.[2] (The English still speak of the movies as "the flickers.") In 1878, near the end of his life, Plateau[3] published an annotated bibliography of the world literature on subjective visual phenomena, including flicker, and it is to that bibliography that we owe most of our knowledge concerning the ancient history of the study of flicker.

The observation that a whirling torch will be perceived as a complete circle of light or as a more or less complete arc of light is an ancient one. That the speed of rotation and the size of the circle are related is also an ancient observation. The fact that a whirling spot of light is perceived as a circle of light gave rise to the idea of persistence of vision—that is, the idea that in some fashion the image of the light persists after the source has moved on. This simple observation served as the starting point for the organization of many other varieties of subjective visual phenomena, such as afterimages, colored shadows, and color mixtures made by whirling disks.

According to Plateau, the first record of an experimental study relevant to flicker is the report written about A. D. 150 by Ptolemy-Regiomontanus, of Alexandria, who in Book II of his *Optics* (which still survives) noted that if a disk is painted with sectors of different colors and then spun rapidly a new and unique color results. Ptolemy also recorded observations on the time of the persistence of the perception aroused by a whirling spark. These later observations were cited by the famous Arab physician Alhazen, who about A. D. 1000 in his *Book of Optics* recorded his attempts to measure the time necessary for perfect vision, together with the speed of movement at which fusion can be perceived.

It remained for Segner[4] in 1740 to be the first to publish actual measurements of the persistence of vision—that is, of flicker-fusion. He found that the persistence of the image of a whirling spark was related to the length of the circumference and that the speed had to be faster than one-half second per revolution for the image of a complete circle to be formed. He arrived at a figure of one-tenth second, or 10 revolutions per second, as the speed necessary for persistence of vision under most conditions. D'Arcy, evidently unaware of Segner's work, reported to the French Royal Academy of Science in 1765[5] an elaborate study of persistence of vision. He made use of a whirling disk with a hole in it, which was illuminated from the rear. The disk was turned by a train of gears actuated by a weight. The speed of rotation could be governed and regulated. He made all his measurements at night—that is, with the dark-adapted eye—and found visual persistence to be about thirteen-thousandths second. Following D'Arcy, the persistence of vision of a whirling spot has been measured by many investigators under a wide variety of conditions, so that the effect of the size of the apparent circle, the brightness of the spot, the size

of the image on the retina, the interaction of two concentric whirling spots, and so on have all been determined.

The experimental study of flicker-fusion may be properly considered to begin with a paper read by Henry Fox Talbot, M.P., F.R.S., before the Royal Society of London on June 19, 1834.[6] In this paper, entitled "Experiments on Light," Talbot formulated the relationship between the period of light and period of darkness as they are combined to provide the level of luminous intensity, which formulation we now call Talbot's law. He said:

> The intensity of light and the time of the bodies remaining at any given point of the circle are inversely proportional to the circumference of the circle they describe; it follows that they must be directly proportional to each other. This suggests a very important idea, namely that *time* may be employed to measure the intensity of light. This seems a very simple law and therefore likely to be true.

Plateau[7] confirmed Talbot's statement by repeating and extending the experiment in 1835.

The second major scientific advance in this field was that which related the frequency of intermittence and intensity of the light, which relation we now call the Ferry-Porter law. This was stated by Ferry[8] in 1892 as "Retinal persistence varies inversely as the logarithm of the luminosity." Porter[9] in 1902 phrased it as "The speed with which the disc must be driven in order that flicker may just vanish varies directly with the logarithm of the illumination of the disc." In addition to Talbot's law and the Ferry-Porter law there are several other regular findings or laws, so that it may be said that from the standpoint of physical optics flicker phenomena have been intensively studied and the findings well formulated in terms of physical laws.

An application of the phenomenon of persistence of vision to development of the invention of motion pictures by Plateau has been mentioned. The next practical application of flicker phenomena was the development of flicker photometry—that is, the use of flicker in the precise measurement of the relative intensity of light. There are many ways to measure the intensity of white light, but there is only one satisfactory way to measure the intensity of a colored light in terms of a white light or in terms of a light of a different color, namely, by flicker photometry. If a test field is illuminated by alternate flashes of two different colors, say, red and green or blue and white, then a point will be found where the observer has no sensation of flicker, but if the speed of either the red or the green is changed but slightly then flicker will be perceived again. In this fashion the intensity of any colored light can be accurately equated with that of a white light of known intensity. This method was first devised by Rood[10] at Columbia University in 1893 and fully developed and standardized by Ives between 1912 and 1918.

In 1888, an elaborate study of visual acuity was reported by König and Brodhun.[11] Most of this study, which included observations on flicker, was done with König as the observer. The findings and the details of the experiments were published in full. Hence, in the days before more objective standards in the form of precise physical instruments, it was possible to compare similar findings to those which had been made with König's eye. In 1902 Braunstein,[12] in Cracow, standardized his eye, so far as flicker-fusion was concerned, in terms of König's eye. Being a practicing ophthalmologist, he measured the flicker-fusion threshold of many of his patients, making and recording a similar measurement with his own eye under the same conditions. In this way he was able to refer the variations found in patients back to the so-called standard eye of König. His study represents the first clinical application of the flicker-fusion method.

Between 1902 and 1935 only an occasional application of the flicker technique to clinical problems reached the literature. Between 1935 and 1940 Hecht and his collaborators extensively studied visual acuity and dark-adaptation.[13] They found that the rate of dark-adaptation was in large part a function of the adequacy of vitamin A in the diet. When vitamin A was given to persons known to have had a low dietary supply of this vitamin, the rate of dark-adaptation or of better night vision was sharply increased. They constructed an instrument called an adaptometer to measure this rate. The adaptometer has been widely used in clinical and public health studies and has a direct relevance to flicker investigations.

Since 1945 there has been an ever-growing interest in the application of flicker measures to clinical problems. The studies of Simonson and Enzer,[14] who have investigated critical flicker-fusion in relation to fatigue, of MacFarland[15] on anoxia, of Krugman[16] on anxiety, and of Krasno and Ivy[17] on cardiovascular pathology are illustrative of this interest. Halstead[18] included flicker-fusion in the battery of tests which he used in constructing an impairment index to indicate the loss of efficiency following injury to the human brain. Finally, Kathleen Young[19] utilized the flicker-fusion test in the first Columbia-Greystone Brain Research Project, finding that under certain specified conditions the change in flicker-fusion following psychosurgery was associated with the regaining of mental health.

It seems worth while to survey certain of the established factual knowledge relevant to flicker-fusion. The physical conditions that give rise to the sensation of flicker or to the sensation of steady illumination may be stated with some precision. Basically involved is a light source which will deliver an intermittent intensity of luminosity to the eye. The source may be a constant beam of light cut periodically by the revolving sector disk, in the same fashion as the revolving blades of an electric fan appear to give flicker and to dim the brightness of a light viewed through them. Or one may utilize some special form of lamp which itself goes on or off completely at a rapid rate when it is properly energized. In either case, if the light is in the range of moderate brightness, and the flashing is maintained at a rate of 20 times on and off each second of time, it will appear to flicker to any observer with normal vision. If the rate of intermittence is raised to 70 times a second everyone will see the light as apparently steady. For every observer, under a defined set of conditions, a transition point or threshold may be found where the flicker changes to steady light or the steady light begins to flicker. This transition point expressed in cycles per second is known as the CFF. In theory it would be interesting to express this critical point in terms of the duration of each flash of light or of each dark period. It would be interesting also to translate this point into an absolute time-space-energy figure. So far as I can find, these expressions have never been worked out systematically. It is customary to express the CFF point in terms of cycles per second of time—for example, 29.2 cps.

The CFF point for any observer may be found between 2 and 80 cps, depending on certain determining conditions. Just what this point signifies depends on the theoretical frame of reference which the investigator assumes. Until late in the nineteenth century the point was considered to be the place where persistence of vision formed or failed. The English physiologist Sherrington[20] in 1897 investigated flicker and developed the idea that flicker was a psychological contrast phenomenon—that is, the presence or absence of contrasting experience of light and of dark. The English psychologist McDougall[21] in 1904 studied flicker in terms of the physiological duration of the retinal impression. Other and more recent investigators have spoken of flicker as the vanishing point of illumination differences, experiential temporal homogeneity versus heterogeneity, time differentiation threshold, and as discontinuous illumination with temporal induction of continuity. Seemingly this experience which seems essentially so simple is open to a variety of interpretations, no one of which is completely satisfactory.

The conditions that cause the CFF to appear at differing rates in terms of cycles per second constitute a fairly long list. We know with certainty that the CFF point is altered by changing the intensity—that is, the apparent brightness of the light. It is altered by the color of the light. It is altered by the ratio between the lengh of the flash of light and of the dark period in each cycle of intermittence. It is altered by the size, shape, and position of the patch of light as it falls on the retina. The state of light- or dark-adaptation of the eye varies the CFF. The length of time the flashing light is observed, say, one second or five seconds, varies the CFF. So also do the relation between the size of the flickering test patch and the size of the pupillary opening, if the patch is larger than the smallest possible pupillary opening; the shape or striation of the area observed and the brightness and color of the immediate surround of the test patch. Whether one or both eyes are used, the body temperature, and the chronological age of the observer act to alter the CFF.

In addition to this list of determinants concerning which we have a fair degree of certainty and understanding, there is a second list of conditions which may, or may not, alter CFF; that is, the evidence is not uniform or complete. We are not sure whether CFF is altered by the color or by the intensity wave form of the flash of light over and above the absolute intensity in energy terms; whether practice in observing in a CFF experiment gives a so-called learning or practice effect; whether sensory stimuli of sound, taste, or smell which occur when the CFF point is being determined actually cause the threshold to be altered; and we are not sure which changes in CFF are primarily related to retinal function alone and which to a combination of retinal and cortical effects. The effect of color blindness on CFF is still obscure. In animal forms, other than man, there is good evidence that the CFF is dependent on the anatomical structure of the eye and on the genetic constitution of the individual, but whether genetic constitution alters human CFF is not known.

Fortunately, most of these determinants can be held relatively constant in any given experiment. If as many determinants as possible are kept constant (and specified), then it is possible to work out the interrelation between any of the major variables. Unfortunately all too many investigators have been unaware of or have ignored some of the

determining variables, and hence their results are often of doubtful significance.

There is still one more difficulty that is frequently ignored. It is conceivable that a relatively fixed point of flicker-fusion exists for any given intensity of light, in the same sense that there is a relatively fixed point of oral body temperature at 98.6° F., provided age, health, and nutritional status are reasonably well equated. A great deal of theorizing about the nature of flicker and the significance of flicker-fusion points has rested on the assumption that it is a relatively fixed figure comparable to body temperature. But such a fixed relationship has never actually been demonstrated, nor has the variability been reported in the CFF point for the same individual measured under the same conditions from month to month, nor for a large group of individuals measured at repeated intervals. Crozier[22] has provided much evidence relevant to the variability of CFF in two or three trained observers, pointing out that the variability of the measure in terms of its standard deviation is quite as important as any other point brought out by flicker studies. Until we have confirmation or negation of Crozier's emphasis on the importance of variability obtained from large groups of individuals, we cannot be certain whether a given individual can be expected to have a relatively fixed relation of CFF to frequency and intensity of light. Until we are more certain of this basic point, flicker-fusion as a method of clinical investigation will be of limited significance. In other words, all an investigator can do at present is to make each person studied his own control, or, if a group of individuals is utilized, then the average change of CFF must prove to be statistically significant.

Three general points are worth further consideration: (1) the variables, which must be specified or controlled; (2) the pathological and pharmacological findings; and (3) the probable psychophysiological significance of CFF in the light of present knowledge. Certain of the variables or determinants which will be considered may seem both obvious and well known. During the past ten years, however, articles have been published in which at least one of these variables was neglected or said to be of no importance.

That chronological age varies flicker-fusion is well known. In part this variation is due to the anatomical changes of the lens of the eye as we grow older. In any event, the determinations made on individuals of various ages should not be averaged. The health and nutritional status of the observer must be known and specified. The speed of dark- and light-adaptation, and hence the CFF, are governed by a complex of relationships, among which are the vitamin A, oxygen, and glycogen level of the blood supply to the retina. The observer should be relatively comfortable during the observations, and there should be a minimum of distractions, such as changing level of noise, illumination, and the like. Intersensory effects are possible and may complicate results.

It is possible to determine the threshold where fusion becomes flicker, or where flicker becomes fusion. The usual procedure is to make a series of five or more measures, both fusion-to-flicker and flicker-to-fusion, and then strike an average of the measures, designating the average as the CFF point. One should be sure, if this procedure is followed, that an equal number of thresholds in each direction have been made, and if any considerable range exists among the individual measures obtained at any given time—say, more than 4 or 5 cycles—the entire procedure should be critically reviewed, since in well-controlled studies the standard deviation of any such combined series is usually reported to be less than 2 cycles. Viewing a flickering light, especially one of high intensity, is an unpleasant experience to most persons; hence, the observer should not be called on to watch a continuous flicker for more than 3 or 4 seconds at a time. Indeed, a well-controlled study usually allows the observer to view the flickering patch for as short an interval as possible—say, 1 or 2 seconds—and then state whether the patch was steady or flickering.

Depending on the purpose of the flicker-fusion determination, one should be careful to control the area of the test patch and to report both its shape and the visual angle it subtended on the retina. If one wants to study only foveal, cone, or photopic vision, the visual angle should be between 1.0° and 1.8° of arc, since at less than 1° and beyond 1.8° complications enter.

The size of the pupillary opening bears a regular relation to the intensity of illumination. To avoid or allow for changes in CFF that are due to alterations in pupillary opening as one changes intensity, several methods are available. One can use a test field which subtends a visual angle at the cornea no larger than the smallest pupillary opening at highest illumination—namely, 2 mm. One can convert the illumination intensity to photon or troland units, which are millilamberts of light per square millimeter of pupillary opening. Or one can use an artificial pupil, which is a mask with a hole in it placed in the line of vision so that the field always has a fixed visual angle below that of the smallest pupillary diameter.

The flicker-fusion point is in part determined by the ratio of the flash interval to the dark period in each cycle of intermittence; hence, one must specify what this ratio is and whether it is fixed or not. If one uses a General Radio Company Strobotac, the light-dark ratio varies with frequency, and the contour found by graphing frequency against intensity has a different position from that where the time of the flash has a constant ratio in flash cycle.

The brightness and hue of the immediate surround of the test patch, as well as near-by patches of flickering or steady light, affect the CFF. If the field is crosshatched or striated, the CFF is higher than when an unfigured field of the same area is used. The research of Werner[23] has shown that the pattern or figure of the field has a pronounced effect on CFF.

Finally, one must be sure to instruct and demonstrate to the observer just what a steady light looks like at each level of intensity, and what a flickering field looks like at each level. Crozier[22] has described the appearance of a white field flickering slightly below the point of fusion at low-brightness intensity as smooth, bluish-grey pulsation, at low-medium intensity as specked with yellow spots, at medium intensity as a granular surface, at medium-high intensity as frosted, and at high intensity as a smooth, pulsing surface. I think it possible that much that has been called "practice," as well as much of the variablity in results, is due to the fact that untrained observers do not know what to look for.

Next we may consider some of the studies that have reported the relation of CFF to the action of drugs, chemicals, and biological agents, and to certain conditions of physical pathology. A survey of the literature indicates that the following agents tend to raise the CFF point—that is, make the visual mechanism more efficient—strychnin, thyroxin, benzedrine, adrenalin, small doses of glucose, dinitrophenol, pervitin, and testosterone. The following agents lower the CFF: chloral, insulin, atropin, nicotin, pilocarpin, excess CO_2, deficiencies in vitamins A and C, and almost any drug or chemical in the barbiturate group. The following agents sometimes raise and sometimes lower CFF, depending on dosage and other conditions: alcohol, caffein, scopolamin, O_2, nitroglycerin, and nembutal. The following have been found to be without effect: saccharin, vitamin B, and neohetramine.

Clinical investigators have reported an altered CFF (usually a decreased threshold) in a wide variety of pathological states, including many ophthalmological conditions, cardiovascular disorders, anemia, brain tumors, brain injury, parasympathetic denervation, nicotin neuritis, hypothyroidism, castration, oligiospermia, beriberi, renal stone, cirrhosis of the liver, combat exhaustion neurosis, depressive phase of manic depressive psychosis, exogenous mental retardation, and "exhausting" illness.

In spite of the fact that both the pharmacological and pathological investigations have been reported by a variety of investigators, using a variety of methods of differing exactitude, these lists do make sense. They indicate that any condition or agent which acts to decrease the available blood sugar and/or oxygen available to the retina or to the brain decreases CFF, whereas conditions increasing the efficiency of the vascular supply increase CFF. In other words, CFF seems to be a measure of the functional efficiency of the retina and/or the visual cortex. This becomes really interesting and promising. But just what is the best measure of flicker-fusion function?

Certainly the easiest procedure would be to arrange things so that all possible determinants were fixed or controlled as exactly as possible, and then to vary the frequency of intermittence and to report the results in cycles per second. This is what a majority of investigators have done, particularly those who have used CFF as a test or indicator of some changed condition. There are several serious drawbacks to this procedure, however. First, there is no standard table of comparison so that one may say that under certain specified conditions and with a certain age range of observers the CFF was in the expected range. Neither are there any accepted variabilities for the assumed normal range, so one cannot say whether a measure is in or out of the expected range. Second, there is good reason to believe that the units in cycles per second mean something different at 15 cycles, at 40 cycles, and at 60 cycles, both as to average figures and as to variability. Third, no one at present can say with certainty what might be the optimum combination of fixed determinant standards, so that an alteration in CFF which may be brought about by a chemical agent or by some other factor is really representative of a change.

Somewhat more complicated measures or indicators have been suggested, among them the highest flicker-fusion point that can be reached, the slope of the line connecting a series of frequency-intensity relationships, the correlation between the highest frequency attainable and the slope of the frequency-intensity relation, the variability or standard deviation of a series of frequency-intensity meas-

ures, and finally the careful plotting of the flicker-fusion points at various locations on the retina in the same fashion as one plots visual acuity or color discrimination with a visual perimeter.

The exploration to determine the highest flicker-fusion point attainable has from a theoretical point of view much to recommend it. This maximum frequency represents the highest attainable speed with which the nervous system and retinal receptors are restored to respond in an overlapping rather than a discontinuous fashion, so giving rise to a sensation of steady illumination. It seems that time should be one of the best of the indicators of the functional efficiency of the nervous system. But from a technical point of view the attaining of maximum frequency is increased in some fairly constant relation to intensity of illumination up to 50 or 60 cycles per second. At some point in this upper range, either increasing intensity has no effect on CFF or it actually decreases CFF. At high luminous intensity the ratio between the period of flash and the dark phase of each cycle, the body temperature, and the area and patterning of the field act in some as yet undetermined fashion to raise the maximum frequency when intensity is held constant. Most investigators seemed to have obtained a maximum frequency somewhere near 60 cps, but Hylkema[24] reached 82 cps, which he said would by optimal correction of the light-dark ratio be 94 cps. However, several investigators have found that if an intermittent electrical current is passed across the closed eye, flicker is reported to continue to some point well over 100 cps of electrical current. Cords[25] combined electrical and visual intermittent stimuli and got a maximum of 172 cps before flicker became steady. The problem is an interesting one, but certainly beyond present direct clinical application.

With two observers, Crozier has thoroughly explored the effects of varying almost all the major determinants in the slope of the frequency-intensity curve. He has also provided the variability and correlation coefficients for his determination. Unfortunately the interrelationships turned out to be complicated, and some of the curves for his two observers are divergent. Just what will be found if his methods of experiment and analysis should be applied to a group of persons cannot be predicted.

The method of flicker perimetry has been used by four or five investigators. That it has a definite place in ophthalmological and neurological examinations seems well demonstrated. As a procedure it is somewhat tedious and demands considerable intelligent cooperation on the part of the observer. I doubt that it will find much use in clinical practice until such time as a relatively simple apparatus is available that will be within the scope of ability and cooperation of the usual ordinary patient.

If the problem is so indeterminant both as to controls, measures, and technical difficulties, why are so many investigators at work on flicker at present? (One European visitor told me that during the autumn of 1950 he found flicker studies going on in ten out of twelve American institutions he had visited.)

The interest in the problem grows out of several recent findings. The cooperative study of semi-starvation at the University of Minnesota[26] reported that flicker-fusion was one of the most sensitive indicators of psychophysiological change found. Krasno and Ivy[17] reported that in a study of cardiovascular patients the absorption of 1/150 grain nitroglycerin invariably lowered flicker-fusion in normal persons, and in 99 per cent of 216 cardiovascular patients it raised flicker-fusion. Furthermore in several patients where other cardiovascular indicators of impairment were not particularly indicative, but where the CFF was increased by nitroglycerin, the patient suffered from a coronary thrombosis during the following six months. Krugman[16] found a lowered CFF in combat air crew personnel who were suffering from operational neuroses, which threshold increased as the neurotic symptoms disappeared. Tanner[27] reported that the light-dark ratio as a determinant of CFF correlated in the + .50s with intelligence as measured by the ACE in two different groups of 25 and 21 subjects each.

Finally, in the first Columbia-Greystone Brain Research[19] Project, nine patients who had a CFF of more than 24 cps were subjected to topectomy and seven of the nine made a social recovery from their mental illness. In the second project only three patients had a CFF of over 24 cps, but in two of the three the operation reduced CFF and recovery from psychosis took place. This, among all the hundreds of indicators or measures employed by all research disciplines in the Brain Research Project was in accord with therapeutic results more frequently than any other objective or semi-objective test employed.

In the original Columbia-Greystone report I wrote as follows: "The decrease in CFF means there must be a greater time interval between flashes of light before there is a functional restoration of some mechanism which when tripped off

sets up the visual process. . . . The operation reduced the speed of restoration of the mechanism."[28] Today I would add that when such a reduction has been effected the chances of recovery following psychosurgery are great.

The retina of the eye is from a neurological standpoint an outgrowth of occipital cortical tissue. It has certainly been demonstrated to be a sensitive indicator of neurological efficiency. Much research remains to be done, but it seems to me the problem is an open one and the promise of solution and practical application is most enticing.

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SMO ON THE AIR

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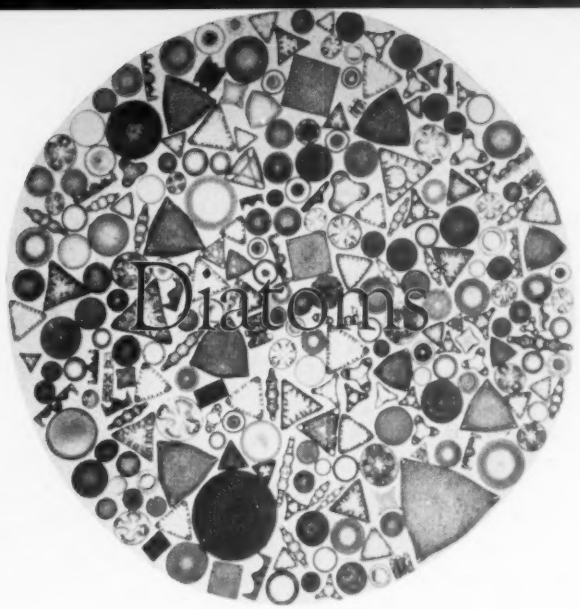

PAUL S. CONGER

The author has been associate curator in charge of the diatom collection at the Smithsonian Institution since 1943 and began his association with the Smithsonian in 1921. From 1922 to 1943 he was with the Carnegie Institution, where he was a research associate for eight years. His research has been concerned mainly with the ecology, physiology, classification, and taxonomy of diatoms, and he here discusses their significant ecological role, which has been largely overlooked.

THE most important role of the diatoms is not any one of the several commonly attributed to them, but an obscure and unknown one which eclipses them all in fundamental scope and significance. These minute organisms perform a service that seems to have escaped general notice, because it is subtle and inconspicuous in its workings and academic in character. Furthermore, although numerous statements have appeared on the economic importance of diatoms, there has been little or no critical examination of the subject that might have led to recognition of this special role. In the past, attention has been directed almost wholly toward the "practical" and more obvious functions of diatoms. It is well now that it be redirected to an aspect or result of their age-old function of photosynthesis, which is significantly combined with their size relations. A résumé and evaluation of the whole subject of diatoms and their importance will provide background and principles that will give their special role appropriate perspective.

In spite of the fact that diatoms are extremely abundant and have been well known to science for more than a hundred years, they have remained surprisingly obscure. Our ignorance is readily explained. Their microscopic size and general invisibility do not encourage widespread attention; their effect upon our personal lives and interests, although tremendous, is so indirect that we seldom become conscious of them; and their beneficent activity is of such a character as to defy human control.

Diatoms seen under the microscope as single, quiescent, colorful plant cells do not convey an impression of power or importance, and we may be inclined to underrate the role they play. The fact is, their small size and plant nature are elements of strength because inherent in the diatom's minuteness is the power to absorb dissolved nutrient efficiently, respond to environmental conditions quickly, and multiply rapidly. In the diatom's plant nature resides its power to use sunlight and convert inorganic nutrient into organic matter by photosynthesis. In its peculiar diatom character resides the power to concentrate special metabolic products of high food value, such as oil and vitamins, and to produce siliceous shells, or cell walls. Its smallness and delicacy also enable many kinds to float in suspension, a feature of utmost consequence in depths of water where other types of plant life, or bulky masses of plant material, cannot thrive.

To appreciate the diatom one needs to acquire a microviewpoint, or microattitude, thinking in terms of small size and immense numbers. The importance of such minute organisms inevitably depends on their occurrence in prodigious numbers, and on the cumulative effect of their products and activities over a period of time. Mass productivity is the first premise of diatom importance, and the

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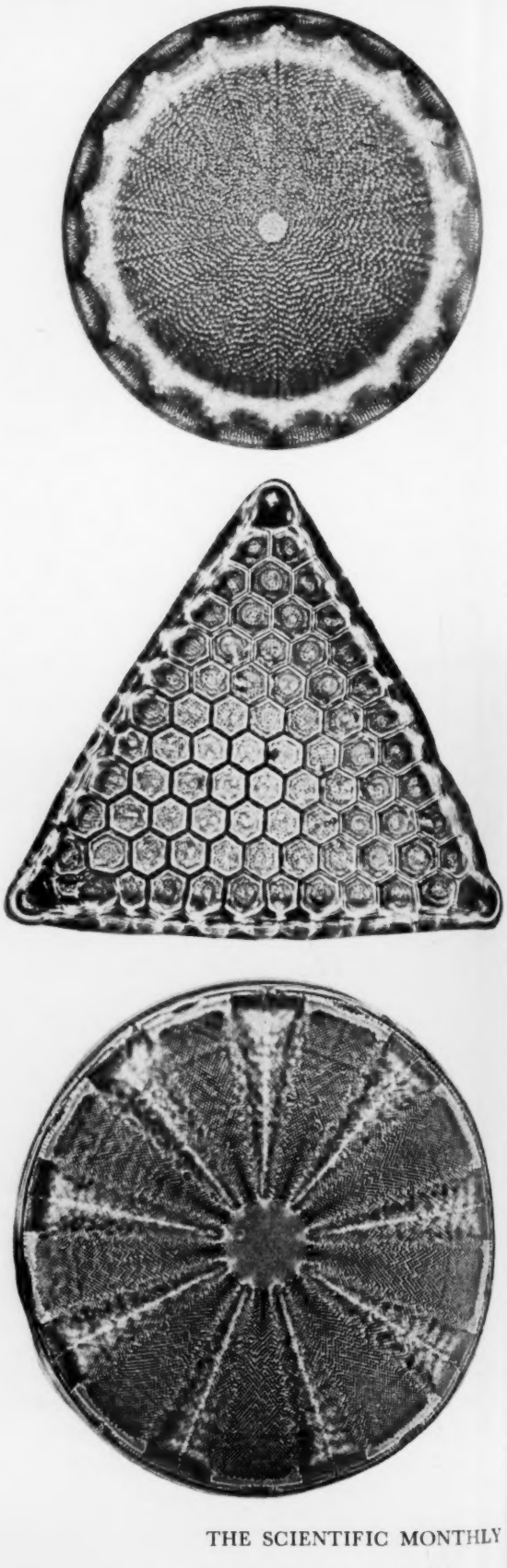
evidence of such productivity seems everywhere at hand. Diatom cells, or spores, which occupy virtually every drop of sea water in the light-penetrable upper layer of the ocean, and which are profuse on bottom and submerged surfaces within this "photic zone," are the ever-ready seed for prolific reproduction when suitable temperature and chemical (food) conditions of the water permit. It is not unusual for them, under the impetus of favorable conditions, to expand their population in a body of water, within a matter of days, or even hours, from a passive state of hundreds or thousands of cells, to several millions, per liter of water. Even when their cells are no more than a millimeter apart, water may still look reasonably clear.

Were this article a sermon, its text might well be, "Blessed are the meek, for they shall inherit the earth." Surely there is nothing meeker than a diatom as seen under the microscope—quiescent, inert, anything but energetic and aggressive. Yet there is scarcely another organism that has more nearly inherited the earth or, at least, "the waters thereof." In lakes and rivers, and in the oceans that cover more than two thirds of the earth's surface, their cells are to be found in practically every liter of water. Still, the diatom's way of life is so modest and inconspicuous that its effects generally escape notice. It is only the most unusual circumstance that brings it to our attention.

In practically all respects the diatom is one of the most efficient of plants. This fact may account, in no minor degree, for its wide distribution and dominance in the oceans, and its prominence in many other aquatic environments.

Before we attempt to evaluate the phases of diatom importance, we must establish the criteria which determine the most important role of an organism. Is it a question of personal opinion, mere statistical magnitude, or an absolute, unalterable relationship, inherent in nature? Should it be measured in terms of direct interest to man, or is it of larger scope?

The relative importance of anything is commonly judged by the degree to which it is either harmful or beneficial to man. Other factors, however, need be taken into consideration besides the mere interests of man, for there are, perhaps, broader viewpoints from which to gauge relative importance. The role of any organism is the part it plays in nature, and, apart from its direct interest to man, is a composite of the ways it affects everything else in its environment. Often, though not always, the



At right, top, *Alacodiscus Oregonis* Bail., × 480; center, *Biddulphia* (*Triceratium*) *Favus* (Ehr.), × 760; bottom, *Actinoptychus splendens* (Ehr.?) Shadb., × 400.

way it most effectively functions in its environment in relation to all other organisms and conditions is, in the long run, coincident with the way in which it is also most important to man, either positively or negatively.

Since this broader viewpoint is obviously ecological, and the matter of importance is relative, perhaps the "most important role" of any organism is that one which most extensively affects the general ecological pattern, or which the organism plays in modifying or determining the general relationships of nature.

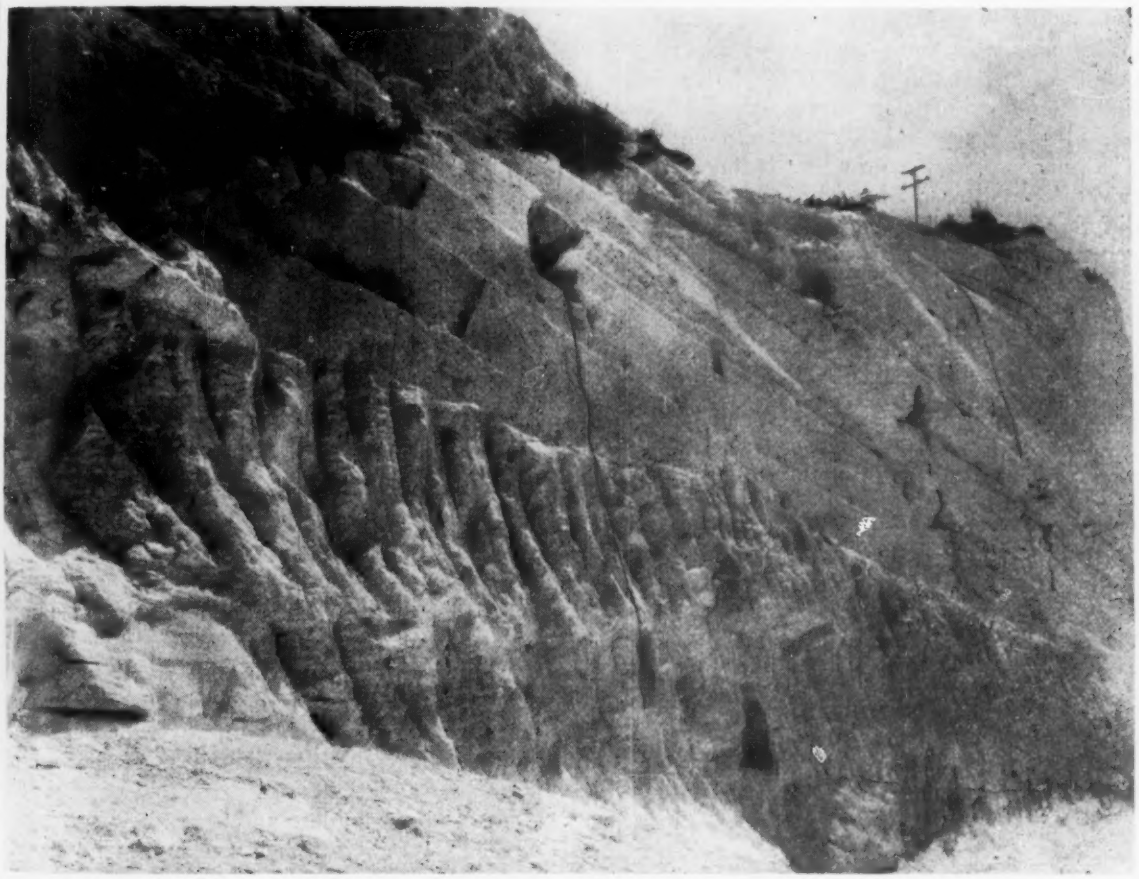
From a philosophical point of view it might be asked whether there is, after all, any such thing as a "most important role," or are there simply so many various roles, one larger and another smaller, but each a coordinated and necessary part of an ecological equilibrium, which we commonly call the balance of nature? In this sense, to point to a most important role might seem a contradiction of the whole idea or philosophy of ecology.

An illustration in point is provided by the so-called trace elements, or growth-promoting substances, which, in respect to quantity of material, seem to have a minor place in plant growth, but nevertheless an exceedingly vital one functionally, and as critically essential as that of the commoner elements comprising the bulk of the plant substance. Without the inconspicuous part performed by any of them the whole ecological pattern would be changed.

Thus, the most important role is sometimes hard to evaluate. It is perhaps less important to emphasize a particular role than to understand how it operates and to conclude that a certain role is larger, or more conspicuous, or has wider consequences.

Diatoms as a Nuisance

The phenomenal reproduction sometimes attained by diatoms in a body of water under particularly favorable conditions may reach such proportions that the individuals actually choke and clog the gills of fish swimming in their midst, causing death of the fish by suffocation. Or, again, under certain conditions, as respiring organisms,



Huge diatomaceous earth deposit of the Johns-Manville Corporation at Lompoc, California. The telephone pole at top right indicates thickness of deposit, and whiteness shows it purity. (Johns-Manville photo.)

their great numbers exhaust the oxygen or carbon dioxide in a body of water, causing the death of many fish by asphyxiation.† Fortunately, the particular combination of conditions that brings about such an event is reasonably rare.

A circumstance of this kind in the geological past may be recorded in the large deposit of diatomaceous earth developed by Johns-Manville at Lompoc, California, where at one level, a thin layer of countless thousands of fossil herring skeletons bespeaks some catastrophic occurrence in the ancient ocean. Similarly, a superabundance of diatoms has been known to clog a city's water filtration facilities, with resulting heavy maintenance cost.

Such events may be so serious as to attract local attention, and they attest unmistakably the potential capacity of the diatom as a proliferating unit.

Environmental Dominance

Diatom fertility, capacity for food consumption, population density, and environmental dominance are, in themselves, sometimes matters of stupendous magnitude, which we may take for granted and fail to recognize as a well-defined and significant effect. The productive capacity for concentration of available food material in one type of plant substance, the diatom, if it had no associated effects, would be in itself a matter of consummate importance. The usurping of an environment by one well-suited type of organism, to the exclusion of others whose different composition and qualities might alter the general ecological picture, is a subtle and important consequence.

† In a comparatively warm, quiet body of water, where dissolved gas retention is low, and where there is not enough agitation to aerate the water adequately from atmospheric contact—especially during the night or during a dark period when the plants themselves are using up oxygen for respiration—an especially heavy growth of plankton diatoms may at times exhaust the oxygen supply to a low point intolerant to fish life. Sudden decomposition of such a heavy plankton crop under these conditions may also seriously reduce the oxygen supply.

On the other hand, under similar water conditions, when little carbon dioxide is being absorbed from the atmosphere, a dense diatom plankton, drawing heavily on the dissolved carbon-dioxide supply for photosynthesis, may reduce the amount of this essential gas to a point where there is insufficient left to serve as a heart and respiration stimulant for fish life, with the result that fish will succumb to asphyxiation in what might seem otherwise an ideal condition of abundance of oxygen.[1]

As has been said, the combination of factors necessary to cause such a catastrophe—warm water, prolonged quiet and stagnation, right light conditions, low dissolved gas concentration, and heavy diatom or phytoplankton population—are, fortunately, seldom present at one and the same time.

Diatoms as Fish Food

Diatoms, as a rule, are utilitarian rather than harmful. They constitute the basic food supply of marine life, and reference to them as the "grass-of-the-sea" well describes their part as the main, often only, forage plant life over great areas of the oceans, and their consequent function in the feeding of many animal forms and, ultimately, of practically all sea life. This function, it should be said, they share with flagellates and some other floating microscopic plant forms[2]—to just what extent is not known, although they appear to be the dominant and most conspicuous form.

In this capacity, they are the necessary agent for converting dissolved substances, generally unavailable to animal life, into tangible, organic, plant food that can be utilized. As such, they are largely the foundation for world fishery resources, amounting to some five billions of dollars a year, to say nothing of the sustenance they furnish to the other kinds of animal life of the sea.

This food interrelationship, of recognizable vastness, is allied with, though still less far-reaching than, their most important service.

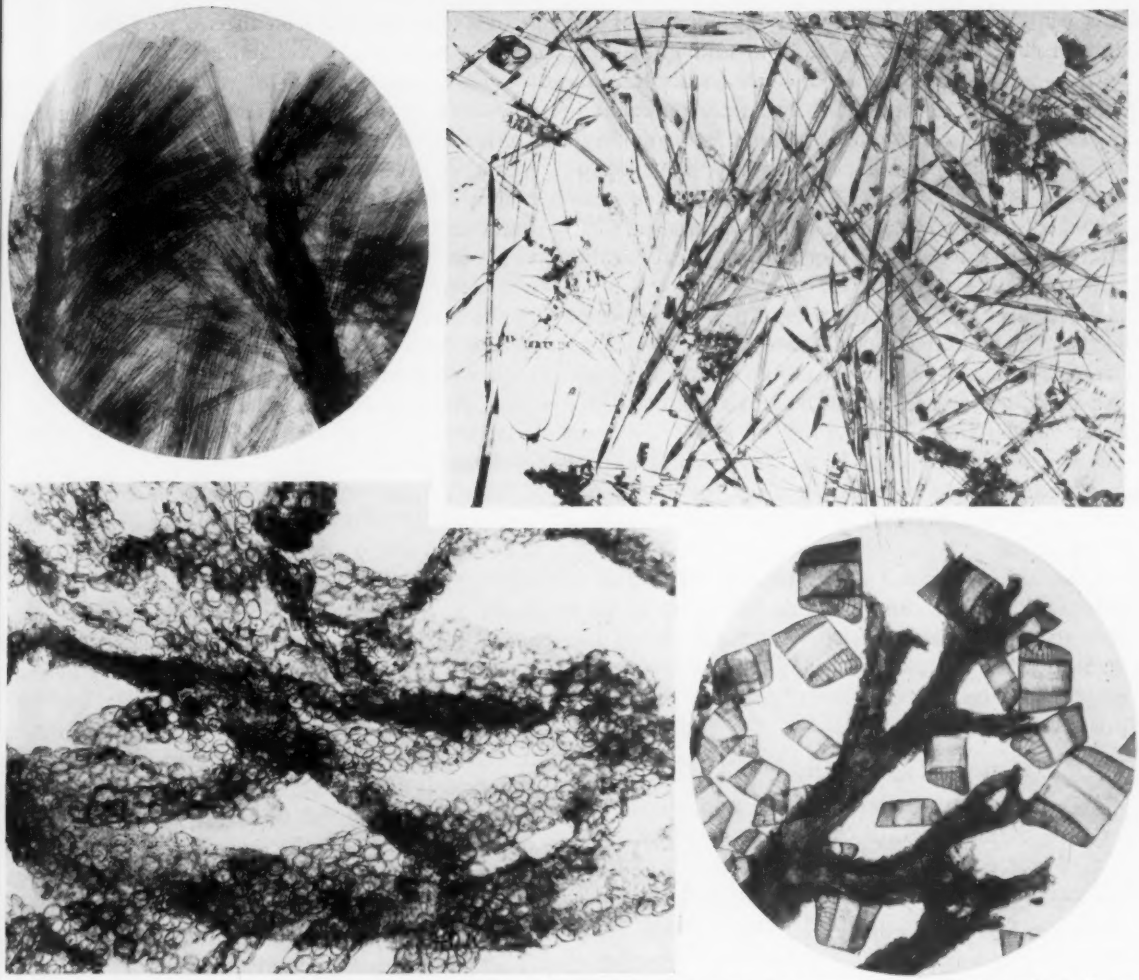
Diatoms and the Formation of Diatomaceous Earth

Diatoms form many thousands of square miles of sediments; the deposits of their siliceous shells remain as diatomaceous earth in many parts of the world and are an economic resource of some importance. Industrial use of hundreds of thousands of tons annually of this uniquely resistant and porous material for high-temperature insulation, filtration of many commercial products, polishes, fillers, absorbents, and a great variety of other uses touches the life of everyone much more extensively than we are aware. This contact with diatoms as a mass material is one of the most frequent means by which many persons gain an acquaintance with them. Clearly a side of their usefulness of great practical interest to man, it is of limited significance otherwise.

Diatoms and Vitamins

People are acutely conscious of the benefit of dietary supplements of vitamins A and D, obtained from the liver oils of various fish, such as cod, halibut, and shark. It is not so widely known, however, that these vitamins and vitamin-bearing oils originate through synthesis by diatoms and other marine plant life, and that the fish merely consume the plants and accumulate and store the vitamin-rich oils. Thus credit for their true origin is, in part, misplaced.

Alert to this relation, chemists of the Dow Chem-



Diatoms shown growing densely on seaweed. Upper left, *Synedra Ulna* Ehr.?; upper right, collection of plankton off coast of New England; lower left, heavy encrustment of *Cocconeis* on seaweed; lower right, *Isthmia nervosa* Kuetz.

ical Company, during World War II, investigated the idea of securing the vitamins from the original source—by-passing the fish and extracting the oil directly from diatoms.[3] The attempt at their plant in North Carolina, where they so successfully extract bromine from sea water, did not prove feasible, however, because the sheer mechanical difficulty of straining oily, gelatinous objects like diatoms, in mass, from vast quantities of sea water was not as simple as the precipitation of a chemical element. On the other hand, fish, not fixed in any one place like a chemical plant, but free to swim continually in search of areas where at the moment diatom growth might be heavy, and possessed of a delicate gill-raker mechanism for separating the solid plant food from the water, could do the job infinitely better; hence it was left to them.

It is but fair that we recognize the photosynthetic activity of countless billions of individual diatom cells as the original agency for creating these vitamins. In fact, it is difficult to imagine a more nearly perfect mechanism than these cells, to act as minute floating chemical factories, effectively exposed in the sun-bathed surface water to the light energy necessary to combine chemical substances in solution into oil and vitamins. Yet the several-million-dollar-a-year vitamin business, in which diatoms play this appreciable part, must be rated secondary to their still vaster nutritive importance.

Diatoms and the Origin of Petroleum

There is reason to believe that diatoms feature prominently in the origin of petroleum, which seems to have originated in shallow-water marine sediments, at depths where diatom growth is prolific. An oil is synthesized by the diatom and stored in its cell as reserve food material in place of such substances as starch or sugar; and the amount thus stored was found by the late C. Juday[4] to be any-

where from 2 to 11 per cent of the volume of the diatom cell.[5] This oil, resistant to and immiscible with water, if not consumed, tends to accumulate after the death of the cells, and may remain enclosed in pockets, migrate in sediments, or undergo various changes. Whatever its destiny, if we postulate the amount of oil which has been thus produced as a mere 1 per cent of the volume of some of the vast deposits of diatomaceous earth, sizable pools of oil are conceivable.

There are two major theories of petroleum origin —(1) that it came from diatoms and marine plant life, or (2) that it resulted from the decay of marine fish and other animal life. Whether it came directly from oil produced by diatoms, or indirectly from fish and animal life that consumed the diatoms as food, utilizing or storing their oil, or both, the diatoms obviously played a rather considerable part in its origin.

It would be difficult either to comprehend or to gainsay the significance of this function, at least to the interests of man, to whom petroleum has been so vital. Yet, however significant this function may have been, it is only a facet in the over-all importance of diatoms.

Diatoms and Aeration of Water

By consuming carbon-dioxide and giving off oxygen, diatoms aerate the waters in which they live. Their contribution in this respect is hard to estimate or determine and is, of course, usually supplementary to other means of aeration, such as atmospheric diffusion and mixing by wind, and it can hardly be considered their most important role. There are times and places, however, in the respiration of a body of water not in motion, when subsurface aeration may be very considerably, even critically, augmented by the diatom and other plant cells in suspension. In a lake sealed from contact with the atmosphere by a covering of ice, suspended plant cells, activated by the light that passes through the ice, may provide a reduced aeration sufficient to maintain the enclosed semidormant animal life through a period of stagnation. Plankton respiration may well contribute an aggregate effect of far more importance than is often realized.

The "Most Important," or Ecological, Role of Diatoms

Whatever significance we may attach to the functions of diatoms thus far discussed, none of them compares with their most fundamental relationship of nature, which, for lack of a better term, we might designate their *photosynthetic,* or *ecological,* role. Diatoms perform their greatest and most far-reaching service as portable photosynthetic agents, with special emphasis on the word "portable." The transformation and fixation of intangible dissolved nutritive material into tangible organic substance, thus making it available for food or sedimentation, are activities they share in common with all green-pigment-containing plants. This is nothing new, and it has been recognized as an important function, basic to the feeding of marine life, since the very early days of diatom study.[9] As the most abundant and most widely distributed plant form in the oceans, diatoms are ubiquitous intermediary agents between bacterial decomposition and the higher organisms needing organic substance for food. As such they are the original synthesizers of a large proportion of the material in the sea. That is elementary.

The crucial points are the fine adaptability of the diatoms to do this, the efficiency with which they do it, *where* they accomplish it, and the great ecological significance thereof.

Photosynthesis and the fixation of material take place quickly and continuously in all locations and levels in the water, in response to the amount of dissolved nutritive matter present at any given place. The operation of the process involves the activity of each individual diatom cell as a functional unit in its own microenvironment, extracting nutrient material from the water immediately surrounding it, and the aggregate effect of infinite numbers of diatoms throughout the oceans. In this concept, the water takes the status, to all intents and purposes, of a continuous metabolic medium, in which closely proximal, invisible diatom cells, as biochemical units, effectively extract the dissolved substances from the water, and the flexibility and mobility of their population permit rapid response to prevailing conditions of nutritive richness.

The bulk of this fixation takes place in the relatively shallow near-shore neritic, or coastal, zone —the area within what oceanographers call the "mudline," roughly corresponding to the continental shelf around most land masses. In this specific geographic locale the rapidly reproducing, abundantly dispersed diatoms and other phytoplankton organisms—in mouths of rivers and coastal waters—intercept all inflowing dissolved materials and constitute an effective *biological barricade* against loss of these valuable materials to the vast spaces of the open oceans.

Long before the waters of a river reach its mouth —in fact almost from their source—the process of transformation and fixation of dissolved materials has been started by the thousands of tiny diatom cells floating in suspension. In the lower reaches of a river, where the tides ebb and flow twice a day,

the turbulence facilitates intimate contact between the plant cell surfaces and the solute in the water. In shallower shore areas tidal movement circulates the water back and forth through dense weed beds, where a profuse epiphytic diatom growth on weed and other submerged surfaces extracts and lives upon the rich nutrient from the water in which it is bathed. Turn of the tides affords a more quiescent period, when some settling of fixed organic substances can take place. In the meantime, marine animals feed upon these now available substances, and the materials are further assured of retention within the area by the animals that can swim and govern their movements. But it is the tiny plankton plants, mainly diatoms, floating everywhere in the water, that first screen out and synthesize the dissolved materials into solid tangible subtstance and attract animal life to the area in which they are living. Their capacity to do this depends upon their minute size, their maximum cellular absorptive surface, their quick growth and rapid multiplication, their subsistence in the solute upon which they live, and their universal dispersion in the water body.

An excellent example of the workings of this process is afforded by the long and indented course of the Chesapeake Bay and its fluvial tributaries. Before the waters of such a river system leave its mouth, the rich nutritive wastes from the land have been in large part arrested, and transformed into new living plant and animal substance, or precipitated into the rich muds of the bottom, where they will undergo further chemical and biological conversion. The relatively small amount that escapes this effective plankton gauntlet will not go far to sea, for it is promptly and similarly dealt with by the coastal plankton and returned by the animals that feed upon it to the shallower inshore waters.

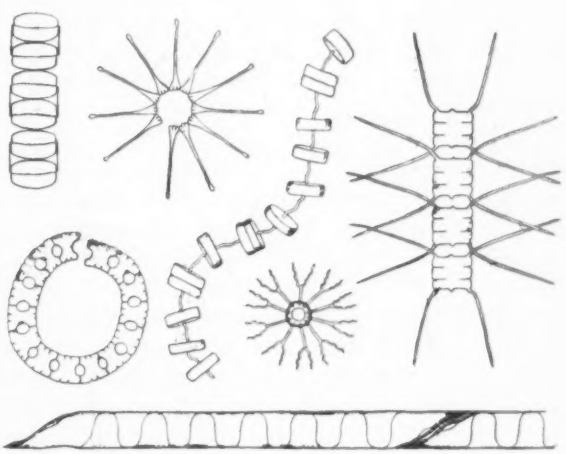
Were it not, however, for the cosmopolitan presence of these tiny floating plants, and their ability to waylay this material near its source, the rich nutritive runoff from the land that so lavishly fertilizes our coastal waters would remain irretrievably in solution and be swept unimpeded into the open ocean, becoming a great homogeneous chemical mixture, ever enriched at the expense of the impoverished land. Yet the dispersal of nutrients throughout the whole ocean would render it so tenuous and unconcentrated as to be virtually lost forever, so far as recovery and re-use as living substance are concerned. As it is, a very scant minimum of such dissolved substance gets by this plankton blockade.

An idea of the efficiency of this hydrological photosynthetic fixation process can be obtained from the following figures: Gran[6-8] found one gram of solid (dry weight) plankton matter per cubic meter in open water of the North Sea, and it has been estimated to be one fortieth as much as the productivity of inshore water. On weed beds in the mouth of the Patuxent River, Maryland, the writer obtained more than 300 grams (dry weight) of epiphytic diatom growth per square meter, two feet deep, an amount about equal to the weight of seaweed itself, representing a very rich production.‡ Another measure of the effectiveness of the fixation process is the difference in dissolved solid content of water near its source, and after it has been subject to plankton growth—a reduction from several or many parts per million of dissolved matter, to a content of the order of a few tenths or hundredths of a ppm. These rough figures give an idea of the comparative richness of in- and offshore waters, as well as the extent to which dissolved nutrient is held in inshore waters by phytoplankton fixation.

The figures were chosen to represent the extreme limits of the range in amounts of dissolved substance, although it is quite likely that there are areas where organic matter content is less than the lower figure, and others where it is more than the higher figure cited.

This ingenious process of nature for halting the loss to the open oceans of eroded products from the land is aided secondarily by a number of other fortuitous dynamic agencies, all of which seem to be working together continually to prevent this waste:

(1) In addition to the part played by the tides, the oceanic currents, which swing along and bear



Greatly enlarged drawings of typical plankton forms show the beautiful structure of diatoms.

‡ This work was done through facilities of Chesapeake Biological Laboratory.

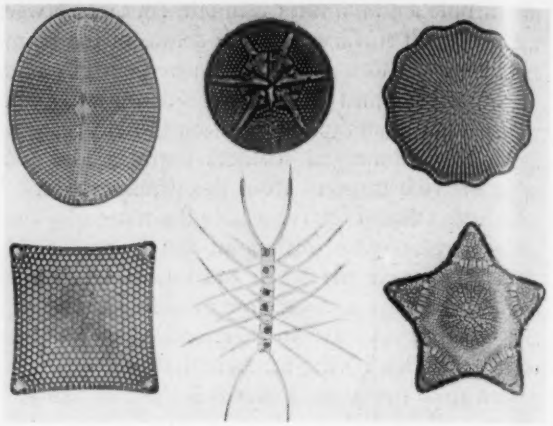
against the continental coasts, tend to prevent the rivers and runoff of the land from pouring directly out to sea with their load of dissolved nutrients, and to divert their waters along inshore areas, where the materials they carry are subject to the activities of the microscopic phytoplankton. (2) The prevailing winds, which blow shoreward from cool, high-pressure areas at sea to warmer, low-pressure ones on land, aid materially by moving surface water layers shoreward, even to pushing the waves upon the beaches and sweeping all waste materials into this zone of high chemicobiological conversion and reclamation.

Other forces, such as up-welling and turnover of water, caused by temperature, density, and pressure variations at different depths, promote vertical circulation, and provide a means for continually bringing up into the surface layer decomposition products of materials that have settled into deeper water, where, below the photic zone of plant growth, they would otherwise be lost.

Ultimately, then, all potentially rich organic material lost from the land by rivers and all forms of runoff—an estimated millions of tons yearly—finds its way sooner or later into the near-shore sphere of activity of the prolific, well-dispersed diatom phytoplankton, where it is fixed and precipitated as a rich organic mud, or is introduced into the recurrent food cycle of re-solution and reconversion into plant and animal substance. In final analysis, the universally present microscopic plankton plant cell (principally diatoms), capable of photosynthesis, is the key to the *in situ* fixation of dissolved nutrient matter in any body of water; and it is the *in situ* fixation that effects its retention near its source. This is the secret of the salvaging and accumulation of the vast amounts of valuable organic substance in the shore and near-shore regions of the earth, instead of irreparable loss at sea.

One result of this continuous dynamic process is the formation along our coasts of a highly concentrated zone or fringe of rich organic matter, representing an accumulation of the best fertilizing elements. Witness such luxuriously rich organic accumulations as the broad marshes of the Chesapeake and Chincoteague Bays, the New Jersey coast, the inland passage along Albemarle Sound and the southern coast, the Gulf Coast bayous, the eel-grass–bordered shores and *Fucus*-covered rocks of New England, to mention points along our East Coast alone. There is perhaps no place where there is a more vigorous biota, a more rapid turnover of life, and a greater accumulation of wealth of organic substance, unless it be in tropical rain forests.

Visible evidence of the effectiveness of this subtle, but potent, ecological arrangement is found in (1) the abundance and variation of fish and animal life in the near-shore waters of the earth, (2) the organically rich bottom-mud deposits of coastal areas, (3) the density of marsh and associated shore growth, (4) the sparseness and poverty of oceanic plankton, as compared with coastal plankton, and (5) the low concentration of dissolved matter, in general, in areas remote from land, as may be seen from tables of analyses in any oceanographic work dealing with sea-water analysis.



These photographs indicate variety of shapes. Upper row, *Mastogloia splendida* (Greg.) Cleve; *Asteromphalus* sp.; *Stictodiscus Eulensteinii* (Grev.) Castr. Lower row, *Biddulphia cuspidata* Janisch; *Chaetoceros atlanticus* Cleve, *Biddulphia juncta* A. Schrn.

Localization of rich organic deposits in near-shore areas is not without exception. Oceanic currents, carrying a heavy load of such wealth, swing away from land and so transport high concentrations and rich growths into the open oceans remote from shore. Here deposits of insoluble organic remains, such as diatomaceous shells, accumulate, but even in these places the soluble constituents return to solution, and a considerable portion of them is carried back eventually to the shore, where they are recaptured and incorporated within the recurrent cycle of life there.

Among the benefits arising from these relationships, the following are noteworthy:

1) The concentration of organic nutrient matter in so definite a fringe along the continental and other coasts draws to it higher forms of life that feed upon the lower, and localizes in the near- and relatively near-shore waters the sea-food and fishery resources of the world, where they are most accessible and can be most conveniently exploited.

2) Large quantities of valuable fertilizing materials, both carelessly and inevitably washed or blown from the land, are not so hopelessly lost in remote and inaccessible depths of the oceans.

3) Proximity of the organic-matter life-zone to the coasts abets the ultimate return of some of this wealth to the land whence it came, by birds as guano, by tidal flooding, by winds, and by still other agencies. Though small, the effect may be more appreciable than may appear.

Very significant ecological effects also accrue from this universal distribution of portable microscopic photosynthetic fixation agents: (1) It assures availability of food substance in varying degree in all waters, and at all levels. (2) It enhances the mobility and distribution of life in the water. (3) It makes practicable the swimming type of life versus a mainly substrate existence, in large and deep bodies of water. (4) It facilitates maximum utilization of dissolved nutrient resources. (5) It renders the ocean and other water bodies highly efficient, productive, and idealistic types of environment.

These relationships and effects comprise the essence of the theory or philosophy of plankton existence. The force and effects here described are not entirely outside the realm of man's influence and utilization. Some control is feasible and will doubtless be taken advantage of in the future. The most practicable approaches are in improved conservational practice and stoppage of erosional loss at the source, pollution abatement, and aquicultural utilization of shallow-water areas. In the latter lie perhaps the most promising pioneering possibilities. Some improvement and extension of means for recovery and return to the land of the near-shore fertile organic wealth are also within hopeful range of practicability. Such benefits, however, are proportionately small, and there is no prospect of any appreciable alteration or subjugation of the whole dynamic process. An understanding of the principles simply brings us to a realization of how fortunately we are favored by these ingenious ecological relationships of nature.

A fine philosophical point is revealed also in the contrast of the highly effective mobile efficiency of the single-celled, or colonial, microplankton type versus the more specialized and highly organized, but much more encumbered, multicellular type of life. In this respect the self-sufficiency of the free-living cell is a superior asset, and its reproductive power is one of the great forces of nature. Strength, as well as flexibility and distributional advantage, resides in the additive effect of numerous workable small units.

The intense concentration of life in the organically rich littoral zone makes for general productivity, rapid turnover, and maximum ecological efficiency. In the life cycle in this rich organic zone there is involved the age-old question of which came first, the hen or the egg; for the vigorous life of the area based essentially on the microflora, microfauna complex is responsible for the fixation and concentration of organic matter, which in turn supports the vigorous life complex.

In this fine biochemical system, the diatom phytoplankton, in its capacity to arrest dispersion and subsequent loss of essentially valuable materials, near their source, is the key factor in a role of the greatest ecological and economic significance. This ecological role is basic to the distribution of life in the sea, the conservation of world resources in a broader sense, and much of the vital economy of nature. It rests upon a very small circumstance—the mobility and adaptiveness of the plankton principle as a facile mechanism, and the supreme fitness of a one-celled (or colonial) floating plant, the diatom, as a photosynthetic agent.

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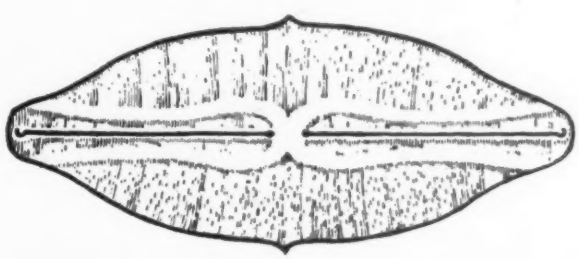

SCIENCE ON THE MARCH

A PRIMITIVE SUGAR-CANE MILL

DURING a recent visit to the island of Dominica, B.W.I., I observed a very primitive sugar-cane mill that deserves more than passing mention. Douglas Taylor, in *The Caribs of Dominica* (Anthropological Paper #3. Washington, D. C.: Smithsonian Institution [1938]), devotes only a short paragraph to its description and makes no further comment, nor does he refer to previous reports. This short note is intended to call attention to the implications of the existence of such a mill and its relationship to the history of fermented beverages and of sugar-cane culture in the West Indies.

Sugar cane is planted occasionally on the island of Dominica, but is not used in the making of sugar. Near the town of Rosseau, the capital, juice is extracted by machinery, using very wasteful methods. Judging by the condition of the bagasse when it is thrown away, a large percentage of the sugar remains in it. The juice is fermented and used for drinks. Dominica is famous for its "limeade" punch.

The Carib Indians, in the Carib reserve in the eastern part of the island—the only Carib reservation in existence—also cultivate sugar cane and chew the stalks, or extract the juice. Each family presses its own cane in a very simple device (Fig. 1).

Just outside the house a thick post is stuck in the ground. A groove is beveled out on one side of the post by means of a 45° cut, meeting a horizontal cut about two thirds of the way across the post. A hole is pierced through the remaining third, and through the hole a thick stick is thrust. This stick is the lever that extracts the juice by pressing against the cane, which rests on a horizontal cut (Fig. 1). The pressed juice trickles down the sides of the groove to a container made from a gourd or a calabash and located in another groove somewhat lower in the post. The juice, called *vesou* by the Caribs, is used as it is to sweeten coffee or cocoa, or it is fermented and later used as a drink.

When I observed this, I took for granted that, primitive as this seemed, no credit could be given to the Caribs, since I assumed that the "mill" must have come from the East with the sugar cane. The *Encyclopaedia Britannica* says that "In India at the present day there are thousands of small mills worked by hand, through which the peasant cultivators pass canes two or three at a time, squeezing them a little, extracting out one fourth of their weight in juice." Later, I found that the mill antedated the introduction of sugar cane in the West Indies. It was used for the grinding or crushing of cassava or other plant materials from which juice for drinks was extracted.

Recently it was reported that such a primitive sugar-cane press is used in some parts of Cuba. Taylor reported (*loc. cit.*) that it is also common in Guiana. This tends to prove its universality in the pre-Colombian population. At any rate, it appears to be the most primitive sugar-cane mill known.

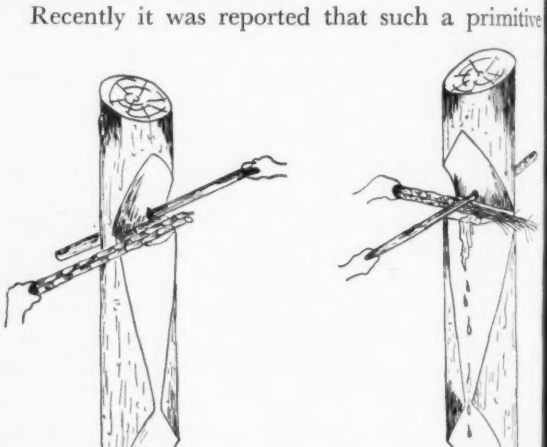

FIG. 1. Author's sketch of primitive sugar-cane mill, B.W.I.

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BOOK REVIEWS

HIGH AND LOW BAROMETER, SWEEPING ROUND FROM PLACE TO PLACE

Watch Out for the Weather. Jacqueline Berke and Vivian Wilson. 226 pp. $2.95. Viking, New York. 1951.

How About the Weather? Robert Moore Fisher. xix + 186 pp. Illus. $3.00. Harper, New York. 1951.

Cloud Physics. D. W. Perrie. 119 pp. Illus. $4.50. Wiley, New York. 1951.

IT IS a safe bet that since the dawn of the spoken word weather has been one of the most popular subjects of conversation. This is to be expected because of the many and important ways the weather affects our lives. Aside from the obvious effects of weather on our outdoor activities, our agriculture, and our heating bill, practically everyone is aware in a vague sort of way of a more subtle relation between the weather and the human mind and body. The way man is affected by weather is understandably a very complicated affair, and it is perhaps for this reason that this important subject has only recently begun to receive the attention it deserves.

In *Watch Out for the Weather* the authors have attempted with considerable success to bring together in a popular book some of the interesting results of scientific studies relating to man's physical and mental well-being to the weather. Some idea of the interesting relationships brought out may be obtained from the following quotations: "Migraine headaches, so-called 'spontaneous hemorrhages,' gall bladder attacks, acute eye episodes, postoperative complications, orthopedic diseases, gastric ulcers, brain abscesses—all have been studied in relation to abrupt, severe weather changes and all appear to respond to the weather in a varying, though unmistakable fashion," or "In fact, the monthly crime curve [New York City] and the monthly temperature curve were practically identical."

Watch Out for the Weather is not, and does not pretend to be, a scientific treatise. In their effort to produce a popular book containing a wealth of information, the authors can be excused for at times seeming to oversimplify and for being a trifle pat in their explanations. For those who may be stimulated by this book to read further in the field, the authors have prepared what appears to be a very good bibliography. The book performs a real service in calling attention to an interesting and rather neglected field for valuable scientific research.

How about the Weather? has been written for the purpose of acquainting the layman with some of the fundamentals of meteorology and enhancing his ability to interpret weather maps and his own observations. The author presents a large amount of information on weather processes and systems, and the layman cannot help but learn a good deal of meteorology from the book.

Although *How About the Weather?* contains a lot of meteorological information, I think that it falls far short of being a sound introduction into the fundamentals of meteorology in that it fails to present anything like an integrated picture of the primary causes of weather. For example, although Fisher frequently deals with the important process of the heating of the earth by the sun, he makes no direct mention of the equally important process of the earth's radiation into space. The rotation of cyclonic and anticyclonic regions is nowhere explained in terms of the earth's rotation. Although in several places the author speaks of the large energies released by the formation of precipitation, he does not explain the equally important energy absorbed in the evaporation of the moisture that formed the precipitation.

It appears that a great deal of effort has been devoted to making *How About the Weather?* extremely light, nontechnical, and painless to read. This effort will certainly not be lost on some of the public, but I feel that many who read the book with a desire to learn will be exasperated by passages such as this one, which introduces a discussion of the differences between heating of the land and heating of the oceans:

> No one has ever made a success of selling homemade mud pies. For one thing, they're unpalatable and indigestible. For another, mud pies are difficult to cook. You can put one under a broiler on a gas stove, but it won't cook thoroughly. The upper crust may be burned to a crisp; however, because heat is not easily transmitted through the pie the filling will remain raw and muddy. Mud pies, then, are definitely impractical.

Those whose tastes are entirely nontechnical may find that the shortcomings discussed above are balanced by the large amount of weather facts and weather lore to be found in the book. In the appendix is a well-rounded list of government publications and magazines and articles which should be useful to those interested in learning more about the weather and in pursuing the subject as a hobby.

The cloud seeding experiments carried out by Schaefer in the fall of 1946 have led to a greatly increased interest in the complicated physical processes that take place in clouds. In view of the increasing importance of this field and the need for a sound text on cloud physics, it is disappointing to find that *Cloud Physics,* by D. W. Perrie, falls far short of filling this need. It appears that much of this book was already written at the time that cloud seeding techniques were discovered. The chapter entitled Induced Precipitation gives the impression of having been inserted into the book shortly before it went to press. This chapter is poorly organized, and although it devotes much of its

attention to miscellaneous cloud seeding tests, it makes scarcely any mention of the fundamental quantitative laboratory experiments of Cwilong and Schaefer which showed that spontaneous nucleation takes place when the temperature is – 39° C or lower.

Although one can forgive the author for the inadequacy of his treatment of cloud seeding in view of the fairly recent origin of this technique, it is difficult to forgive him for omitting the discussion of such a fundamental physical property as the liquid water content of clouds, which has been the subject of many good investigations both here and abroad. An examination of the subject index shows the following subjects to be conspicuous by their absence: snow, hail, lapse rate, convergence, divergence, condensation, evaporation, adiabatic, inversion, etc. This reflects an inadequacy both in the indexing and in the text.

Cloud Physics is a handsome book, with many excellent photographs of clouds and a good description of their forms and classifications. The author presents some interesting qualitative discussions about the physical properties and processes in clouds, but he fails to undertake anything like the detailed and quantitative treatment that his ambitious title leads the reader to expect. A far better title would have been merely "Clours." A book deserving of the title "Cloud Physics" will have to be more rigorous, longer, and contain much more physics than *Cloud Physics*.

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LAST OF THE ALCHEMISTS

Paracelsus—Magic into Science. Henry M. Pachter. x + 360 pp. Illus. $4.00. Schuman, New York. 1951.

ONE of the most controversial figures of the Renaissance era was the physician Theophrastus, known as Paracelsus. Pachter has written a biography of this fascinating figure and has compiled an interesting treatise on his life and times. Some idea of the content of the book may be obtained from the subtitle, which is "being the true history of the troubled life, adventures, doctrines, miraculous cures, and prophecies of the most renowned, widely traveled, very learned and pious gentleman, scholar, and most highly experienced and illustrious physicus, the Honorable Philippus Aureolus Theophrastus Bombastus ab Hohenheim, called Paracelsus, doctor of both medicines and professor of theology, also adept of the Holy Cabbala and expert of the alchemical art, friend of the common man and defender of liberty. He cured patients whom ordinary doctors had abandoned for doomed, and he knew wonderful elixirs that restore youth to the old."

From this alone it is obvious that Paracelsus was an unusual person. The impact of his life affected his contemporaries as well as subsequent generations. Pachter traces his early life and background in some detail and compares the relation between his theories and those of the physicians and healers of the times. Their effect on the changing ways of medical practice is noted.

It is difficult to estimate the importance of this unusual man, since many proclaimed him a quack, whereas others thought him to have the most profound mind in the history of German culture. It has been said that he served as the model from which the character of Faustus was drawn. His work touched the fields of chemotherapy, psychiatry, physics, and many other branches of the medical arts.

The story is told against the background of changing conditions of the Renaissance period, and as biography and history it is a valuable contribution in its field.

There are numerous illustrations in the text, and the entire work is well annotated.

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SCIENTIST, PHILOSOPHER, AND EXPLORER

Powell of the Colorado. William Culp Darrah. ix + 426 pp. Illus. $6.00. Cumberlege, Oxford University Press, London; Princeton University Press, Princeton, N. J. 1951.

OF THAT small but select group of scientist-explorers who made history as members of the pioneer surveys in America's West during the latter half of the past century, none was more colorful or more diversified in his interests and endeavors than Major John Wesley Powell. Few, if any others, had such effect on the development of a large section of our country or influenced the minds of so many people. It is not surprising, therefore, that William Culp Darrah selected Powell from many outstanding scientists for biographical study.

Major Powell's association with the Colorado River and certain of his basic contributions to our knowledge of geology and ethnology are known to every student of these subjects. Much has been written about him and his adventures; several of his monographic reports are too well known to need mention; yet until now he has stood in the minds of most people as a romantic but legendary figure, and the story of his life has been shrouded by controversial legends and undocumented assertions. Like most men of character and determination, Powell had both strong supporters and bitter enemies, so reports on his personality and accomplishments are in many cases conflicting. In Darrah's book, therefore, an excellent job of checking details, of documenting with footnotes, and of presenting both sides of debatable issues is very much to the credit of the author.

The chronological approach used in this biography is especially appropriate, because Powell's life seems to have been divided into very definite phases. Each of these is distinct, yet related, in that events in one explain developments in another. For instance, the story of his boyhood background in farming, his struggle to get an

education, and above all his insatiable love of natural history, make very understandable his later success, despite adversities, in exploration. Likewise, his experiences as a soldier in the Civil War explain why he used the methods that he did in leading men on hazardous and arduous expeditions. Finally, a knowledge of the combination of all these experiences shows why he was able to develop a philosophy that resulted in great contributions as a director of scientists and as an administrator influencing government policies.

Although its title is misleading, the chapter called "Among the Cliff Dwellers" is one of the most significant. It clarifies a commonly confused picture of scientific accomplishments on the first Colorado River trip, referring only to source materials written before the time of the second. It describes the hardships of the first expedition, which were largely due to lack of advance information, and it contrasts the personnel of the two trips. It emphasizes a point, realized by few, that Powell never regarded the second trip as a canyon exploration but rather as a routine surveying job, and thus it explains many remarkable features relative to the conduct of the second trip. Finally, it describes Powell's methods of carrying on scientific work, bringing out clearly the wide range of his interests.

To a greater or lesser degree, various other chapters contribute to an understanding of Powell as a man, as a scientist, and as a philosopher. The general conclusion is that this book not only gives an interesting account of the life of an outstanding individual, but also accurately portrays the record of evolution in the scientific thinking of a great naturalist. It explains the influence of one man in establishing two great institutions —the United States Geological Survey and the American Bureau of Ethnology.

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ECONOMIC WELFARE THEORY

Social Choice and Individual Values. Kenneth J. Arrow. xi + 99 pp. $2.50. Wiley, New York. 1951.

THE matter of socioeconomic norms cannot be neglected in any discussion of economic welfare theory. Kenneth J. Arrow uses the symbolic methods of the logic of relations to attempt to unravel what he believes to be the contradictions implicit in most discussions of economic welfare theory that are based on purely economic considerations rather than on socioeconomic norms.

Although the author is remarkably clever in manipulating logical symbols in his discussion of the basic principles of economic welfare theory, he is somewhat reluctant to draw the conclusions that his logical analysis should be expected to yield. While admitting that "the doctrine of 'enlightened self-interest' would be justified in the event that it is assumed that all individuals have the same ultimate ends for the community," he recognizes that this assumption is not easily verifiable, and hence that there is a resulting uncertainty as to the relations between social decisions arrived at by individuals and the social ends of the community. Arrow fails to analyze this problem; he simply recognizes that it exists. Since this uncertainty is precisely the rock on which economic welfare theory in the past has been so often shipwrecked, it can only be hoped that in future work this author will shed more light on the subject.

The use of symbolic methods of the logic of relations rather than the more customary mathematical methods of the calculus is a rather novel and refreshing aspect of the volume. Although the work is designed for the general reader as well as for the specialist, the former may find it helpful in conjunction with his reading to consult George Boole's *Laws of Thought* and Lewis and Langford's *Symbolic Logic,* both recently reprinted in convenient format by Dover Publications. Without the aid of such introductory works on symbolic logic, the nonmathematical reader would find himself hopelessly bogged down in symbolic notations, in spite of the author's conscientious endeavors to explain in simple fashion each use of symbolic logic as it is employed.

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SCIENTIFIC A B C

Science and Common Sense. James B. Conant. xii + 371 pp. Illus. $4.00. Yale University Press, New Haven, Conn. 1951.

THROUGHOUT his book the author calls attention to the importance of "timing" in scientific discoveries. The same may be applied to the publication of books. This one is very timely because of the quandary in which the average layman now finds himself with regard to science. For years he had seen Science apotheosized, chiefly, it is true, by nonscientists of the H. G. Wells type, but also by some real scientists. More recently he has been rudely jolted from a state verging on idolatry of science by such debunkings of false claims as Standen's *Science Is a Sacred Cow.* As a result he does not know what to think. This book by the president of our oldest university and the chairman of the board of our newest scientific organization, the National Science Foundation, should set him straight and make him an enthusiastic, but intelligent, supporter of science.

The reasons it is calculated to do so are: (1) as its title indicates, it is based on common sense; and (2) it tells in a simple and interesting way the story of some of the scientific discoveries by which we have arrived at our present state of scientific development. Regarding the first, the author begins by making "certain commonsense assumptions," such as the reality of objects, the uniformity of nature, which the layman has always taken for granted but which certain metaphysically inclined scientists have confused him by denying. He also removes a big difficulty by making a clear distinction between "facts" and "conceptual schemes" derived

from them. Finally, by going against the majority opinion of scientists that there exists *a* "scientific method," quite unique to science and absolutely different from all other methods of gaining knowledge, Dr. Conant makes the layman feel that, after all, science is not such an esoteric affair as he had been led to believe.

The "case histories" of important scientific discoveries are well chosen and are developed in such a way that the layman can not only understand them but also can see that scientists are not different from other men in the way they go about their work. They are just as human and as full of prejudice as the rest of mankind. Furthermore, the author shows how many great discoveries in science were due more to "hunches" than to carefully planned and executed experiments.

In conclusion Conant shows the relationship of science to industry and human welfare and makes a strong plea for public support of basic research through the National Science Foundation. If the book is as widely read as it deserves to be it will go far to dispel the false ideas both for and against science that are prevalent today. And though written primarily for the layman, the book should be read and meditated upon by every true scientist.

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THE ETERNAL SEARCH

The Rise of Scientific Philosophy. Hans Reichenbach. xi + 333 pp. $3.75. University of California Press, Berkeley. 1951.

THIS book, Professor Reichenbach tells us, was "written with the intention of showing that philosophy has proceeded from speculation to science," and also "to inquire into the roots of philosophical error and to present the evidence that philosophy has risen from error to truth." The author holds that the reason so many philosophers seem to speak "an unscientific language" is that they attempt to answer questions at a time when "the means to a scientific answer are not at hand." Yet it is nowhere made clear in the course of the book just what "scientific philosophy" is, and at best we are told what a "scientific philosopher" is: he is one who knows some modern science. One gathers, therefore, that he uses the empirical and "hypothetico-deductive method" of the sciences, and if they are not applicable to problems in philosophy, those problems are discarded as not being within the province of "scientific philosophy." Ethics is thus to be rejected. Says Reichenbach:

> Whoever wants to study ethics, therefore, should not go to the philosopher; he should go where moral issues are fought out. He should live in the community of a group where life is made vivid by competing volitions, be it the group of a political party, or of a trade union, or of a professional organization, or of a ski club, or a group formed by common study in the classroom.

In the modern world this precept will not be difficult to follow; it would, indeed, be more difficult to do the reverse.

Reichenbach rightly attacks such examples of obscurantism as "Reason is the substance from which all things derive their being;" but it is difficult to follow some of his own statements—e.g., "Explanation is generalization." It is also curious to find the author of a book that is couched almost entirely in historical terms exclaim at the end of it, "Those who work in the new philosophy do not look back; their work would not profit from historical considerations." Reichenbach very generously admits, "I do not wish to belittle the history of philosophy; but one should always remember that it is history, and not philosophy. Like all historical research, it should be done with scientific methods and psychological and sociological explanations." Since the armory of the "scientific philosopher" appears to contain the results of science, I presume that the history of "scientific philosophy" embraces the history of science, especially since Professor Reichenbach includes so many episodes from the history of science in this book. As a historian of science, I must admit to a prejudice in favor of having the facts correct to begin with, and I cannot see how the sociological and psychological explanations and the scientific methods have any point if this is not the case.

But Reichenbach's history of science is a collection, in large part, of traditional misinterpretations and errors of fact to a degree that is inconsistent with the degree of reliance on historical evidence. A generation of research on pre-Greek science is ignored completely. The decimal system is hardly a discovery of the Arabs. It is true that many writers do date modern science from the time of Copernicus and Galileo, but, if they do, they usually think of the transition not so much from Greek science to modern experimental science, as in terms of the rejection of the medieval concepts of "substantial forms" and "real qualities." The revolution we associate with Galileo had been in preparation for a long time, and we cannot say simply that men turned to experiment because Galileo lived in a time during which "the result of a change in social conditions . . . freed the minds of the scientists from preoccupation with Greek science in the form of scholasticism and led naturally to an empirical science." Reichenbach declares that the "experiments in which he [Galileo] established his law of falling bodies have determined the pattern of a method that combines the experiment with measurement and mathematical formulation;" but the evidence is plain to anyone who reads Galileo that he did not arrive at his laws by experiment and that the supposed experimental test to which he put his laws was so far from precise that a convinced Galilean who repeated them was forced to admit that Galileo could not have performed the experiments and produced the results he had described. Torricelli hardly "invented the barometer," and it was Pascal, not Torricelli, who "demonstrated that air exerts a pressure which diminishes as the altitude increases." More important than Kepler's

laws for the "universal assent" to the Copernican system was the series of discoveries made by Galileo with his telescope. Dalton did not arrive at his atomic theory by carefully measuring the ratio by weight of combining substances and discovering that a fixed proportion calls "for an atomic explanation;" on the contrary, he deduced the law of constant proportion from his atomic concept, and we know that great chemists—e.g., Berzelius—rejected the conclusion (even more important than the law of constant proportions was the law of multiple proportions, which Dalton also deduced from his concept). Nor are historical errors confined to older science. We are told that Rutherford "connected these chemical discoveries [of Mendeleev] with the discovery of the electron and constructed the planetary model of the atom," whereas it was actually the results of studying radioactivity and the experiments on the scattering of alpha and beta particles that led Rutherford to his concept of the atom.

Save for a short excursus into evolution theory, the medical and biological sciences are not discussed at all in this book, which is limited to mathematics and the physical sciences—chiefly physics. The historical picture would have been quite different had some attention been paid to the nonphysical sciences in every period under discussion. When one reads that "the science of the ancients" is embodied "in their physics and astronomy and mathematics," therefore, one is led to wonder whether the ancients had no biology, whether the biology of the ancients is not to be considered as a science, or whether biology as a whole is not a science. And in one place, where biology is mentioned in connection with Aristotle, Reichenbach attacks one phrase concerning generation, without at all appreciating that Aristotle's conception of epigenesis was far and away in advance of anything that was to come for many centuries.

It seems a pity that a work that contains so much about the history of science and that is devoted to replacing "error" by "truth" should itself attempt to find truth by repeating error.

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WILDLIFE MANAGEMENT

Practice of Wildlife Conservation. L. W. Wing. 412 pp. $5.50. Wiley, New York. 1951.

THE laborious, painstaking, and often underpaid work of biologists in the United States, recently stimulated by Pittman-Robertson funds, is beginning to yield valuable dividends, as evidenced by the appearance of several comprehensive books on wildlife management, most recent of which is this excellent treatise by Wing. The late Aldo S. Leopold doubtless would have been particularly pleased with this book by his first doctorate student, because it so successfully summarizes current data into a sound philosophy for wildlife management.

The book is written in an extremely well-organized and readable style, which avoids scientific jargon and documentation. Each subject is treated briefly, but adequately, inasmuch as there is an excellent selection of references for further reading at the end of each chapter. Progressive sportsmen, private, state, and federal administrators, and university teachers will all find much of interest throughout the book.

Environmental aspects of wildlife management are emphasized, and present two primary tasks for the manager: (1) maintaining as high a population of wildlife as possible consistent with other uses of the land; and (2) deciding how, when, and where to harvest the biological surplus.

Challenges are presented in the book for farmer, forester, rancher, and game specialist. For example, the problem of coordinating forestry practices with wildlife production is clearly stated. The primary job of the forester is production of timber by the development of intensive practices on the best tree sites, whereas wildlife production can be emphasized on the poorer tree sites. Silvicultural practices that can be employed to these ends include preserving natural openings, designing and preserving mixtures of tree species, and working out practices for making frequent light cuts over large tracts in preference to heavy cuts on limited areas.

Grazing men will not find the book particularly flattering. Overgrazing in forests, woodlots, and swamps is mentioned in numerous places as a nemesis to wildlife, but only once are the *benefits* of *light* grazing stated. The winter range problem of big game in competition with domestic livestock is stressed particularly. The author does, however, recognize that elk are a serious problem on range lands, and that intensive, omnivorous grazing habits, destruction of fences and haystacks, and trampling of moist soil by elk often create more serious problems than those associated with domestic livestock.

Of interest to game specialists and sportsmen will be the philosophy of predation. Repeatedly it is stated that predators have little to do with ruination of game populations, and that they are primarily the sanitary and selective mechanisms of nature whereby the unfit, the old, the sick, and the crippled are destroyed. The real challenge to game specialists seems to be that of developing management procedures for fur bearers and waterfowl, since both groups are not up to potential production.

Chapters follow a logical sequence, and each is practically a complete unit in itself. Monetary and other values of wildlife are developed first, followed by a general discussion of physical, nutritional, and psychological needs of wildlife. Then techniques are briefly discussed for describing the environment and wildlife populations and for studying the habits of individual animal groups.

The general relation of farm game to cultivated land and life histories of the most important species are next discussed. Tabular summaries of food and cover preferences and biological characteristics of most important game species are given. The discussion of each species includes habitat requirements, food habits, populations,

and census methods. This same kind of treatment is given for forest wildlife including game birds, small-game mammals, and big-game mammals; for open range game, both birds and mammals; for southern wildlife, which border on the tropical regions; for northern wildlife, which live in the boreal and tundra regions; and for fur bearers. Waterfowl and other wildfowl, lake and stream fish, and songbird and nongame wildlife management are described more generally without discussing individual life histories. A short chapter on extinct, rare, and persecuted species is included. The remaining chapters are devoted to the legal status of game and regulation and administration of wildlife by state and federal governments.

In writing about such a large and complicated field the author will probably not please everyone. For example, some suggestions (which are not meant to detract from the over-all excellence of the book) are: Why was a chapter on Canadian regulations included when they parallel so closely U. S. procedures? With predation such a controversial subject, why wasn't one of the most controversial animals, the coyote, treated in life-history detail as was the black bear? Why weren't rodents as wildlife of the open range given more attention? In borrowing grazing management techniques of range survey for big game, why weren't range condition and utilization concepts presented in lieu of the less useful forage acre method?

The author is to be particularly commended upon outlining the legal status and objectives of wildlife management that must be undertaken in a democratic society. In summary these are: (1) protection of the public interest, of which wildlife is a part, including the obligations of society to provide all forms of wildlife for future generations even though no immediate economic gain appears possible; (2) provision for control over the human element in harvesting and otherwise disturbing wildlife; and (3) assuring each individual an equal political opportunity to exercise the privileges of citizenship with respect to wildlife.

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SARTONIAN CYCLE

Osiris, Vol. IX. Georgius Sarton, Ed. 658 pp. Illus. $8.00 unbound; $9.00 bound. St. Catherine Press, Tempelhof, Bruges. 1950.

IN ANCIENT Egyptian mythology Osiris son of Geb and Nut, the earth god and the sky goddess, had been the husband of his sister, Isis. In modern times both of them are serial publications in a companionship created by George Sarton for the delight of the historians of science. *Isis* has been visiting us quarterly since 1913; *Osiris*, with his lengthier studies, has been calling irregularly, once or twice a year only, since 1936.

Volume IX of *Osiris* appeared after great delay, owing to World War II and the difficulties of printing in Belgium. Each volume of this serial has been dedicated to a famous historian of science. The ninth is in memory of Max Meyerhof (1874–1945), who distinguished himself both as an ophthalmologist in Egypt, his adopted country, and as a historian of Arab medicine. His brief biography and a bibliography compiled by Joseph Schacht introduce the book (pp. 7–32).

There are twenty-one other studies—all in English except Taton's paper—written chiefly by American scholars, on many topics of historical interest: on ad-Damírí's zoological dictionary (Somogyi); on Gaspard Monge and his differential equations (R. Taton); the lunar mansions (P. Yampolsky); Sydenham's philosophy of science (R. M. Yost); a Babylonian philosopher of science from Hammurabi's times (N. M. Baiikey); Swineshead and medieval physics (M. Clagett); the wheel of Aristotle (I. E. Drabkin); Pletho's calendar reform (S. Gandz); Hindu agricultural history (F. S. Hammett); history of bread in classic Greece (N. Jasny); seventeenth-century science (H. Whitmore Jones); cicadas in China (G. K. Liu); social darwinism (G. McConnaughey); Browne as a zoologist (E. S. Merton); plague in sixteenth-century Scotland (C. F. Mullett); Brunelleschi as an inventor (D. Prager); two Arab scientists from the twelfth century, al-Asturlábí and as-Samaw'al (F. Rosenthal); Giambattista Vico and Descartes (G. de Santillana).

Three studies are by George Sarton himself. One of his papers is a short history of the decimal systems, with brief remarks on the nondecimal systems (binary, octonary, and duodecimal). The second is a very remarkable study of the scientific and educational literature written and published in the little-known Romansh language, a national Rhaeto-Romanic language still used unofficially in Switzerland. The third study is a description of the activities of the Carnegie Institution of Washington in the field of the history of science (pp. 624–38), a sort of final report of Sarton's historical "subdepartment" in that institution, which was sentenced to future suppression (to come in 1949) soon after John C. Merriam retired in 1938 and the new president, Vannevar Bush, radically changed the policies of the Carnegie Institution.

Volume IX of *Osiris* contains also a general, cumulative index to all nine volumes because the editor thought that this would be the last of the series. Indeed, our *Osiris* could scarcely have hoped to escape the fate of its Egyptian namesake had it not been for the intercession of the Belgian St. Catherine (the press), which saved the life of the serial. (According to the Sacred Books, the Egyptian Osiris died, either by drowning or by murder, and his body was swallowed up by the Nile.)

Word reached us recently that the tenth volume has been sent to press, and that *Isis* would continue to enjoy companionate marriage with *Osiris*. We wish that a new kind of "Horus" would be forthcoming from the noble wedlock, to complete the Sartonian cycle of valuable historical publications in science.

CLAUDIUS F. MAYER
Index-Catalogue, *Surgeon General's Library*

BRIEFLY REVIEWED

A Field Guide to the Shells of our Atlantic and Gulf Coasts. (2nd ed.) Percy A. Morris. xix + 236 pp. Illus. $3.75. Houghton Mifflin, Boston. 1951.

IN THE past five years, Percy Morris' guide to the sea shells of our Atlantic coast has become the most popular reference work of its kind. In addition to the attractive illustrations, there are clear descriptions, interesting habitat notes, and a wise choice of species. The revised edition corrects the serious defect of the first printing by the addition of the author's name after each species. Six new plates and 112 species have been added, so that an excellent series of the more common species is now represented.

It would perhaps have been better had the author omitted the dates after the genera, for a large proportion of these are erroneous. From the arrangement of the families and the nature of other mistakes, it would appear that Mr. Morris copied his information from C. W. Johnson's obsolete "List of Marine Mollusca of the Atlantic Coast" (*Proc. Boston Soc. Nat. Hist.*, **40**, [1934]). Had the readily available literature of the past ten years been used, a number of synonyms, obsolete names, and spelling errors could have been avoided. Consultation with a malacologist prior to publication might also have reduced such errors as illustrating the operculum of the *Busycon* whelk with that of a *Polinices* moon shell in the first edition, and in the second edition using that of a *Murex*. Despite these minor drawbacks, the book is the finest of its kind and will certainly serve as a useful identification tool for the many thousands of American shell enthusiasts.

R. T. A.

Sir Thomas Brown. Jeremiah S. Finch. viii + 319 pp. Illus. $3.50. Schuman, New York. 1950.

THE 1600s sparkled with scientific stars—Galileo, Newton, Boyle, Huygens, Fabricius, Sylvius, Harvey. It was an age also of much social unrest—especially in England—and of a great stirring in the minds of men. In the midst of the intellectual and social revolution Thomas Brown, physcian, writer, monarchist, and gentle Anglican, exemplified the tug between the metaphysics of preceding ages and the empirical mechanism of this new age of probing.

Thus the merit of Finch's book from a scientist's viewpoint lies not in the details of Sir Thomas' life and writings, but in the all-too-brief sketch of the social scene and its relation to the upsurge of scientific investigation. The author sets forth no specific hypothesis, however, and we are left, accordingly, to construe the relationship for ourselves from his infrequent discussions of the problems of the age and his delineation of the gruelling uphill grind of struggling scientists. The book will probably be welcomed mainly by collectors of books by and about Sir Thomas. Some interesting anecdotes about practices and superstitions in the medical arts of the time are related, but these are of so cursory a nature that it seems unlikely that physicians familiar with the history of medicine could gain much from them.

Finally, since this is another in "The Life of Science" series, the avowed purpose of which is to acquaint the general reader with scientific developments in the history of science, it seems unfortunate that so much space has been devoted to unimportant details in the *life* of the scientist and so little to the *history* of his science. Although the two matters are not unrelated, one may still write a great deal of the one without throwing much light on the other. This book suffers greatly from this practice.

P. B.

Ugo Benzi. Dean P. Lockwood. xvi + 441 pp. $8.00. University of Chicago Press, Chicago. 1951.

UGO BENZI of Siena was a physician of the early fifteenth century in northern Italy. His son, Socino, wrote a biography of his father which was preserved in a Latin manuscript. This was probably the first complete biography covering the life of a physician, and it inspired the first independent history of medicine. The author has drawn upon that biography and all other available sources of material and has written a scholarly biography of his subject. This book is a valuable addition to the bibliography on medical history and represents the results of twenty years of research by the author.

A. E. H.

Freedom and Culture. Compiled by Unesco. Introduction by Julian Huxley. 270 pp. $3.75. Columbia University Press, New York. 1951.

IN THESE perilous times, when freedom is threatened in many areas of the world by totalitarian aggression, it is refreshing to read the Unesco compilation entitled *Freedom and Culture*. Consisting of brief chapters contributed by distinguished writers and publicists, among whom are Jean Piaget, Lyman Bryson, and German Arciniegas, the volume points up the inseparability of culture and freedom and the dangers that are now threatening their very existence.

Penetrating analyses of the essence of both culture and freedom, and its relationship to forms of government, education, and the creative arts, are lucidly presented. The thesis of the book is succinctly epitomized by Arciniegas in the following passage:

"The superiority implied by democracy over aristocratic, oligarchic or absolutist systems lies in offering people of every colour, social origin or race, opportunities of access to institutions working for the advancement of culture through the medium of the sciences, the arts and letters. These opportunities are nowadays a human right on which all intellectual progress depends. . . ."

Works of this nature under Unesco auspices on fundamental human rights are necessary for the free world in its never-ending fight against the enemies of freedom everywhere.

H. P.

Parasitic Infections in Man. Harry Most, Ed. x + 229 pp. Illus. $4.50. Columbia University Press, New York. 1951.

THE fourteen papers included in this monograph were originally presented at the Fourth Symposium of Microbiology held at the New York Academy of Medicine, March 15–17, 1949. The authors are highly competent investigators, and each contribution is an authoritative coverage of the subject assigned. The titles range from "The World Health Importance of Parasitic Diseases" to "The Treatment of Schistosomiasis." The editing has been very well done, and the book is highly recommended. It is to be hoped that future monographs in this series will appear more promptly, as the present volume loses a great deal by the two-year delay in publication.

M. S.

Science: Its method and its Philosophy. G. Burniston Brown. 189 pp. Illus. $3.50. Norton, New York. 1951.

IT IS generally accepted that the average man would find it extremely enlightening to gain an understanding of modern scientific methods and philosophy. It is not often, however, that a scholar of the reputation and stature of G. Burniston Brown essays to explain, in simple and understandable language, what he conceives the essence to be.

In tracing the development of scientific method from its earliest beginnings, the author discusses forms of learning processes in animals, analyzes words and their significance for the growth of science, and describes the achievements of such geniuses as Aristotle, Francis Bacon, Newton, and John Stuart Mill in advancing the scope of scientific method.

The author is very adept in popularizing the topics under discussion, but he occasionally tends to oversimplify difficult subject matter. This is particularly noticeable in his chapter on "ultramodern" scientific philosophy. Professor Brown fails to realize the dangers inherent in generalizations that neglect to take into adequate account the views of opposing schools of philosophers.

Nevertheless, in spite of such defects, the book is rich in human interest and sparkles with its joy in man's intellectual conquests.

H. P.

ON LOOKING INTO SIMPSON'S *EVOLUTION*

From ultramicroscopic molecules
of genes, enzymes, and virus floating free
eons ago in the dark pre-Cambrian sea,
through all phyletic forms in stagnant pools,
slow on the earth, deep down in bathmic schools,
in air, on peaks, wherever life may be,
clear protoplasm flowed resistlessly
to end in us—savants and saints—and fools.
But wait. . . .Slowly emerging man, not beast
entire, not human yet, still struggles on,
dumbly upstrives, and soon will be released
from fear, stand free and proud in some new dawn
endowed with knowledge, science-won, that leads
to life, triumphant over lesser breeds.

READ BAIN

Department of Sociology
Miami University
Oxford, Ohio

LETTERS

IN DEFENSE OF DOODLING

I HAVE just finished reading the very interesting article by John Crawford Pierce on the Fibonacci Series (*Sci. Monthly*, **73**, 224 [1951]). Aside from his expressed scorn for the study of "the relations of numbers with each other" and "indefensible number doodling," an attitude which cannot find favor with a mathematician, I wish to point out another way to determine the constant treated in the paper and to show how successively better approximations may be obtained. The determination of the constant is a matter of plane geometry and elementary algebra, and the sequence of approximations may be found in the chapter on continued fractions in any treatise on higher algebra, such as Weber or Chrystal, and many others.

If the dimensions of the original rectangle are taken to be 1 and $1+x$, and those of the rectangle in the partition, x and 1, we may, according to the assumptions of the problem, set up the equation

$$\frac{1+x}{1}=\frac{1}{x}$$

or

$$x^2+x-1=0.$$

The roots of this equation are $\frac{-1+\sqrt{5}}{2}$ and $\frac{-1-\sqrt{5}}{2}$, of which only the positive one is here applicable. The number .618034 in the article is an approximation to the positive root of this equation.

Certain well-known theorems on continued fractions state that the successive convergents yield increasingly close approximations to the required number, being alternately too large and too small, and that the approximation obtained from a continued fraction is the best that can be obtained at a given level of complexity.

It is convenient to expand $\sqrt{5}-1$ and then divide the successive approximations by 2. We write

$$\sqrt{5}-1=1+\sqrt{5}-2=1+\frac{1}{\sqrt{5}+2}=1+\frac{1}{4+\sqrt{5}-2}$$

$$=1+\frac{1}{4+\dfrac{1}{\sqrt{5}+2}}=1+\frac{1}{4+\dfrac{1}{4+\sqrt{5}-2}}=\cdots$$

$$=1+\cfrac{1}{4+\cfrac{1}{4+\cfrac{1}{4+\cdots}}}$$

The first few convergents are:

$$1, \frac{5}{4}, \frac{21}{17}, \frac{89}{72}, \ldots.$$

We divide by 2 to give the values involved in this problem and obtain

$$\frac{1}{2}, \frac{5}{8}, \frac{21}{34}, \frac{89}{144}, \ldots.$$

In conclusion, may I commend to author and editors the value of "impractical number doodling" and "mathematical gymnastics"?

F. C. OGG

Mathematics Department
Bowling Green State University

THE article by John Crawford Pierce on the Fibonacci Series inspired us to examine our sunflower patch raised from seeds from Burpee's, and we found heads with 55 and 89 spirals; some had 89 and 144 spirals, and one huge head had 144 and 233 spirals.

We were delighted to discover that this biggest sunflower verifies the statement in the article regarding the relation between sunflower heads and the Fibonacci Series, and carries it one step further.

MARGARET K. and DANIEL T. O'CONNELL

The Red House Place
South Londonderry, Vermont

Some of the mathematical properties of the Fibonacci Series, described by John Crawford Pierce in the October 1951 issue of THE SCIENTIFIC MONTHLY, may be of interest to your readers.

The essential property of the Fibonacci Series is that $f(x+2)=f(x)+f(x+1)$. Each number in the series is the sum of the two preceding numbers. It is necessary to designate the two initial numbers (or any other two numbers). If $f(0)=0$, $f(1)=1$, then the series is formed as follows:

x	0	1	2	3	4	5	6	7	8	9	10	11	12
$f(x)$	0	1	1	2	3	5	8	13	21	34	55	89	144

which is the series Professor Pierce described.

The first, second, and all higher order differences of the Fibonacci Series are Fibonacci Series. The general term and the sum can be found. Charles Jordan presents the general term in his *Calculus of Finite Differences* (Budapest 1939).

In the series above,

$$f(x)=\frac{1}{\sqrt{5}}\left(\frac{1+\sqrt{5}}{2}\right)^x-\frac{1}{\sqrt{5}}\left(\frac{1-\sqrt{5}}{2}\right)^x.$$

The numerical value of $\frac{1+\sqrt{5}}{2}$ is 1.618034; the numerical value of $\frac{1-\sqrt{5}}{2}=-.618034$. The proportions of the esthetic rectangle discussed by Pierce were, it will be recalled, 1.618034:1, which equals 1:.618034.

Let us write the equation in the form

$$f(x)=C_1 r_1^x+C_2 r_2^x.$$

Then, r_1 and r_2 are the two roots of what Jordan calls the characteristic equation $r^2 = 1 + r$. This equation is suggested by the equation $f(x+2) = f(x) + f(x+1)$.

The constants C_1 and C_2 are defined by the first two (or any other two) numbers in the series. For instance, if we desire a series in which

$f(2) = 3$, $f(5) = 10$, then $C_1 = \frac{25 - 3\sqrt{5}}{20}$, $C_2 = \frac{25 + 3\sqrt{5}}{20}$,

and $f(0) = 2\frac{1}{2}$, $f(1) = \frac{1}{2}$, $f(2) = 3$, $f(3) = 3\frac{1}{2}$, $f(4) = 6\frac{1}{2}$, $f(5) = 10$, etc.

The sum of any set of consecutive terms in the general Fibonacci Series is:

$$\sum_a^b f(x) = C_1 r_1^{x+1} + C_2 r_2^{x+1} \Big]_a^{b+1}$$
$$= f(b+2) - f(a+1).$$

For instance, in the Fibonacci Series, when $f(0) = 0$, $f(1) = 1$, the sum of the Fibonacci numbers from $f(1) = 1$ to $f(7) = 13$ is $f(9) - f(2) = 33$.

Other interesting relationships exist among the numbers in a Fibonacci Series. For instance, a familiar trick is to have a subject select two numbers $f(1)$ and $f(2)$. The subject then computes the series through $f(10)$ and summates the ten terms. The subject tells the operator the value of $f(7)$ only. The operator then knows the sum of the ten terms, which is $11f(7)$. This problem was posed as part of a more general problem by A. L. Epstein in the *American Mathematical Monthly,* November 1947. The solution is based on the fact that

$$\sum_1^{4k-2} f(x) = (r_1^{2k-1} + r_2^{2k-1}) f(2k+1).$$

In the given problem, $k = 3$.

NATHAN JASPEN
Instructional Film Research Program
School of Education
The Pennsylvania State College

MULTIPLY BY TEN

I DEEPLY regret an error in my article on "The Early Work of the National Bureau of Standards" in the September 1951 issue (*Sci. Monthly,* **73** [1951]). The figures given on page 168 should obviously all be multiplied by 10. The correct values are 299, 790; 299, 776; and 299, 790 km/sec, respectively.

LYMAN J. BRIGGS
National Bureau of Standards

FIFTH ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

Entries in the Fifth International Photography-in-Science Salon, sponsored annually by THE SCIENTIFIC MONTHLY and the Smithsonian Institution, should be sent (prepaid) to the Editor, *The Scientific Monthly,* 1515 Massachusetts Ave., N.W., Washington 5, D. C., Nov. 1–Nov. 27. All accepted photographs will be shown at the AAAS meeting in Philadelphia Dec. 26–31, and later throughout the United States. For entry blanks and full information write to *The Scientific Monthly,* 1515 Massachusetts Ave., N.W., Washington 5, D. C.

ASSOCIATION AFFAIRS

AAAS POLICY

A MEETING was held September 13–15 at Arden House, near Harriman, New York, at which the Executive Committee of the AAAS, together with a group of invited consultants, considered the basic policy and program of the AAAS. The questions studied had been previously set forth in a statement printed in SCIENCE (**114**, 246 [Aug. 31, 1951]).

The Arden House meeting gave consideration to a variety of detailed aspects of the present program of the AAAS and to the ways in which these might or should be modified. But the chief aim of the meeting was to discuss fundamental questions of purpose, policy, opportunity, and obligation. Once a clear and agreed framework of policy is established—and really not until then—effective approach can be made to the problems of instrumentation of policy.

It is impossible to report adequately in a brief space the full content or value of this important meeting. The conclusions tentatively reached are purely advisory in character. The group unanimously adopted a summary statement. In its formal meeting on September 16, the Executive Committee of the AAAS accepted the report of the conference of the preceding days and instructed the Administrative Secretary to publish, both in SCIENCE and in THE SCIENTIFIC MONTHLY, the summary report of the Arden House Conference.

It is most earnestly urged that every member of the AAAS study this statement carefully, and that every member who approves it, or who wishes to suggest additions, deletions, or changes in emphasis, will write his opinion to Howard A. Meyerhoff, Administrative Secretary of the AAAS, 1515 Massachusetts Ave., N. W., Washington 5, D. C. Our Association should approach these important issues in democratic way; and this places an obligation on each individual member.

At the next annual meeting, in Philadelphia, this matter will be considered by the Council of the AAAS. If this statement of policy appears to be generally approved by the AAAS, or if a satisfactory substitute statement can be produced, the Executive Committee plans then to set up a series of committees to study scientifically the practical problems of implementing the policies.

The statement of the Arden House Conference is as follows:

A STATEMENT OF POLICY FOR THE AAAS

Whatever its obligations to other groups and whatever its opportunities in wider fields, the AAAS is an organization of scientists for science. The AAAS must, first of all, serve scientists and science in such a way as to command the confidence and backing of the scientists of this country. Otherwise it will be in no position to meet its wider opportunities.

This central principle indicates the necessity that the AAAS re-examine those of its activities which relate primarily to the internal affairs of science in this country, improve these activities, and extend them when and where that seems desirable in view of present circumstances. This must in particular involve a most careful review of the program and policy in respect to meetings, publications, service to scientific societies and similar groups, aids to research, and various aspects of the interrelations of science and government.

There should be explicit mention of one particularly important aspect of this internal problem of service to science. We have reached the stage where one over-all organization cannot effectively deal with the intensive and specialized interests of individual branches of science. The technical papers that present detailed results in chemistry, in physics, in mathematics, in zoology, etc., can more properly be presented before meetings sponsored and arranged by the appropriate professional groups.

It is thus clear that the AAAS should not attempt to hold to a pattern of annual meetings that was natural and effective many years ago, but which is now outmoded.

This is, in fact, only one aspect of an important general principle. In view of the present size and complexity of science, in view of the seriousness and importance of the relation of science to society, and in view of the unique inclusiveness of the AAAS, it seems clear that this organization should devote less of its energies to the more detailed and more isolated technical aspects of science, and devote more of its energies to broad problems that involve the whole of science, the relations of science to government, and indeed the relations of science to our society as a whole.

This increased emphasis on broad problems should lead to new activities in wider fields, but it also requires a modification of what the AAAS tries to do within and for science. Thus it seems clear that a major present opportunity for the AAAS within science is to act, in all ways that promise useful results, as a synthesizing and unifying influence. As an obvious example, this indicates meetings at which one branch of science is interpreted to the other branches of science, meetings at which are stressed the interrelations between the branches of science, meetings which cultivate borderline fields, and meetings at which the unifying theme would be central problems whose treatment requires the attack of several disciplines.

This opportunity to try to "put science back together" seems so important that it may be wise to modify the existing statement [quoted in the next paragraph] of the purpose of the AAAS to include more specific dedication to synthesizing activities. Such activities are, of course, wholly consistent with the present statement of purpose; but if in fact this is, as some of us feel to be the case, the major present internal opportunity of the AAAS, then it deserves explicit statement.

Finally, this same emphasis on over-all problems demands that the AAAS not only recognize but attack the broader external problem of the relation of science to society. It seems to us necessary that the AAAS now begin to take seriously one statement of purpose that has long existed in its constitution. To quote:

> The objects of the American Association for the Advancement of Science are to further the work of scientists, to facilitate cooperation among them, to improve the effectiveness of science in the promotion of human welfare, *and to increase public understanding and appreciation of the importance and promise of the methods of science in human progress.*

It is clearly recognized that the diffusion among the general public of knowledge about science and its methods is a difficult, slow, and never-ending job. It would require staff, money, patience, and wisdom. It would involve failures, and it would at some points strain the professional sensitivities of scientists. But in our modern society it is absolutely essential that science—the results of science, the nature and importance of basic research, the methods of science, the spirit of science—be better understood by government officials, by businessmen, and indeed by all the people.

We enthusiastically reaffirm our belief in the statement quoted just above as the culminating object of the AAAS; and we favor the adoption, after suitable study, of activities in this field as a major active interest of the AAAS.

It is recommended that this tentative statement of general policy be placed before the whole membership of the AAAS; and that it be sent to all the members of the Council, accompanied by a request for serious consideration and response. As soon as democratic procedures indicate that the above statement, or some then available modification of it, represents a consensus, the Executive Committee should set up a series of committees to study the practical and detailed problems of implementing these principles.

These studies themselves should be carried out in a scientific manner with disregard of vested interests, with tempered concern for traditional procedures, and with imagination in respect to the present and future. Various aspects of the studies can doubtless be usefully aided by the methods of operations research, so that judgments can be at least partially founded on fact, as well as on opinion.

Executive Committee of AAAS	*Special Consultants*
Roger Adams	Philip B. Armstrong
Detlev W. Bronk	J. H. Bodine
John R. Dunning	A. J. Carlson
E. B. Fred	Mervin J. Kelly
Walter S. Hunter	John E. Pfeiffer
Paul E. Klopsteg	Ralph A. Rohweder
Kirtley F. Mather	E. C. Stakman
Howard A. Meyerhoff	Marshall H. Stone
Fernandus Payne	Alan T. Waterman
Paul B. Sears	
Warren Weaver	

Arden House, September 15, 1951

It might perhaps profitably be said that the members of the Arden House Conference did not intend that this statement be viewed as a polite rephrasing which suggests only minor changes. This statement calls for four main things:

1. A real strengthening of the direct usefulness of the AAAS to scientists and to scientific societies.
2. A shift in emphasis from the more detailed aspects of the various technical branches of science to the broader problems of science as a whole.
3. The cultivation of synthesizing and unifying activities as the main emphasis of the AAAS in its internal work within the body of science.
4. The undertaking of attempts to improve public understanding of science as the main external emphasis of the AAAS.

Warren Weaver

For the Executive Committee
American Association for the
Advancement of Science

THE SECTION ON INDUSTRIAL SCIENCE

Science has long since lost its cloistered detachment and "purity," and in a few of the scientific disciplines industry now claims as many graduates of our colleges and universities as education and government combined. These specialists have, however, insisted they are industrial *chemists,* or economic *entomologists,* or petroleum *geologists;* and they have been slow to recognize the significance of the industrial ties that bind them in a common fraternity. In 1936 the AAAS Council considered these ties of sufficient importance to provide for a Section (P) on Industrial Science, but the projected section never came to life. Chemists insisted they were chemists, and geologists were geologists, with as much finality as "pigs is pigs." In the Association's new constitution, adopted in 1946, Section P was quietly omitted, and no one missed it.

In 1950 the American Industrial Hygiene Association became affiliated with the AAAS, and its officers asked whether a separate section might be created to encompass the varied yet closely integrated field interests of such a group. The Executive Committee at once saw the possibility of utilizing an organization of this kind as the nucleus of a Section on Industrial Science, and the question was placed before the Council in a ballot distributed by mail on April 27, 1951. In the returns, one hundred and ninety-one Council members favored establishment of the section; only seven voted against it.

On July 1 the Executive Committee declared the new section in existence on the basis of the returns, and to give it substance, officers and a skeleton section committee were appointed. Allen D. Brandt, of the Bethlehem Steel Company, whose active interest was in no small part responsible for AIHA affiliation, was named 1951 chairman of the section and vice president of the AAAS; and N. V. Hendricks, of the Esso Laboratories at Linden, New Jersey, was chosen as secretary. Frank A. Patty, of

General Motors Corp., had already been selected by AIHA to represent this organization on the AAAS Council and was ex officio a member of the Section P committee. To this trio of industrial health experts was added Paul E. Klopsteg, to serve through 1951.

This skeleton committee assembled in Pittsburgh on August 15 to fill out its membership and to evolve a section program for the Annual Meeting at Philadelphia. Edward R. Weidlein, president of Mellon Institute, Allen T. Bonnell, vice president of Drexel Institute of Technology, and J. A. Hutchison, director of research at Westinghouse's Research Laboratories, were picked as committee members, and all agreed to serve. Their appointments became official at the Arden House meeting of the Executive Committee last September 15–16.

For the Philadelphia debut of the section as a functioning unit within the Association, the officers and committeemen saw no merit in being modest. Installation ceremonies, with Detlev W. Bronk officiating and Edward R. Weidlein as principal speaker, will start the new organization on its way Friday morning, December 28. The American Industrial Hygiene Association will pick up the program Friday afternoon, and on Saturday morning, with H. Thomas Hallowell, Jr., presiding, the section will look at industrial research through the eyes of the following speakers:

ROBERT E. WILSON
Standard Oil Company (Indiana)
"The Petroleum Industry"

HILLAND G. BATCHELLER
Allegheny Ludlum Steel Corp.
"The Steel Industry"

J. B. FISK
Bell Telephone Laboratories
"The Communications Industry"

NORMAN A. SHEPARD
American Cyanamid Company
"The Chemical Industry"

E. H. VOLWILER
Abbott Laboratories
"The Pharmaceutical Industry"

Saturday afternoon will be spent peering into the future, with John S. Zinsser, of Sharpe & Dohme, Inc., in the chair and with four speakers, who will discuss prospective developments from the experience and outlook acquired in their own diverse organizations:

LILLIAN M. GILBRETH
Gilbreth, Inc.
"Horizons in Industrial Science from the Viewpoint of the Consultant"

JAMES CREESE
Drexel Institute of Technology
"Horizons in Industrial Science from the Viewpoint of the Educator"

FRANK C. CROXTON
Battelle Memorial Institute
"Horizons in Industrial Science from the Viewpoint of the Research Institute"

ALAN T. WATERMAN
National Science Foundation
"Horizons in Industrial Science from the Viewpoint of the Government"

Sunday morning, December 30, Edward Hopkinson, Jr., of Drexel and Company, will preside over a session devoted to a consideration of the public aspects of industrial research, in which C. L. Emerson, Georgia Institute of Technology, and G. Edward Pendray will be participants. This program will conclude the section's Philadelphia activities which, like a college graduation ceremony, may be considered a commencement of equally significant things to come in 1952.

On October 11, the section committee met in Philadelphia to preview its December program, to nominate candidates for the chairmanship in 1952 and for the position on the section committee that will be vacated by Dr. Klopsteg, and to outline a program of activity for 1952. In August, only 28 of the Association's members had registered a primary interest in industrial science, and a single society (AIHA) had asked to be affiliated with the section. Another society has now indicated an interest in affiliation, and members, who are currently paying dues for 1952, will soon give Association headquarters some idea of the extent to which Section P can count on membership support.

The officers of the section are not, however, looking for strength among the older AAAS members, but rather in the staffs of research laboratories—among industrial scientists who may see the value of an organization that is concerned less with the technical aspects of corrosion, of food preservation, or agricultural raw materials for new plastics, and more with the problems of management, financial support for research, personnel, team research, access to technical communications, instrumentation—in short, those problems of organization and operation that confront the scientists who are applying scientific discoveries and methods to industrial operations and products.

Not the least of these problems is that of intercommunication with and among specialists in different fields. Here, the Association with its fifteen other sections in every branch of science and applied science, provides facilities for intercommunication and cooperation that are not available elsewhere. In 1952 these facilities will be exploited to an exceptional degree. The American Society of Civil Engineers has invited U. S. organizations to

join with it in celebrating a centenary of engineering. In response to the invitation the AAAS proposes to make "the contributions of science and mathematics to engineering and industry" a dominant theme at its St. Louis meeting in December 1952. Every section has much to offer in developing such a subject; but Sections M and P will carry the heaviest programming burden, even though much of it will be concerned with the coordination of the sessions arranged by other sections. For many years the Association's Gordon Research Conferences have provided a striking demonstration of the interdependence of basic research and industry in chemistry. In 1952 it is confidently believed that an equally convincing demonstration of comparable interdependence in every field can be provided at the St. Louis meeting, which was conceived months before the Arden House Conference, but which is being planned in the spirit of that conference. Although it may be asking a lot of the newest section to join with Section M in giving substance to the major theme of the 1952 meeting, the enthusiastic response of industrial scientists to Section P in the year of its birth is ample guarantee that this lusty infant can assume important responsibilities a year hence.

HOWARD A. MEYERHOFF
Administrative Secretary, AAAS

NEW PRIZE IN SOCIOLOGY

The AAAS announces a prize competition for essays reporting upon inquiry into theoretical sociology. Amount, $1,000.

Conditions of the Competition

A. It is expected that the essays submitted will strive toward a scientific account of the behaviors of human beings, psychologically and sociologically, in their global setting.

B. Contestants should avoid the descriptions of sociological events in terms of conventional and fictional components derived from everyday speech.

C. To be avoided also are statements derived from traditional philosophies which represent only self-created descriptions projected into the social life of people in the guise of objective interpretations.

D. Since it is the goal of the competition to further the comprehension of the psychological and cultural behavior of persons in current society, it is desirable that the descriptions and interpretations be derived from actual sociological situations. Established knowledge bodily transferred from even the nearest and best oriented sciences cannot substitute for sociologically interpreted findings.

E. As concrete suggestions for the study, it is hoped that entrants will aim at sociological constructs to parallel those which have proved so useful in the biological and physical sciences—for example, particles and waves in physics, blood "milieu" in physiology, genes in genetics.

Manuscripts, which will be judged by a committee of three persons selected from those interested in the field, should be sent to Howard A. Meyerhoff, Administrative Secretary, American Association for the Advancement of Science, 1515 Massachusetts Ave., Washington 5, D. C. In order that the judges may be impartial, the name of the author should be omitted from the typed copy.

The committee reserves the right to withhold the prize if no worthy essay is submitted.

The donor is willing to offer the prize in 1951 and to continue it on an annual basis for three more years. This year, in view of the lateness of the date, consideration will be given to articles published during the year, provided they meet the conditions stated above. Ordinarily, preference will be given to unpublished manuscripts. To qualify for the 1951 prize, manuscript material should be in the hands of Howard A. Meyerhoff not later than *December 3.*